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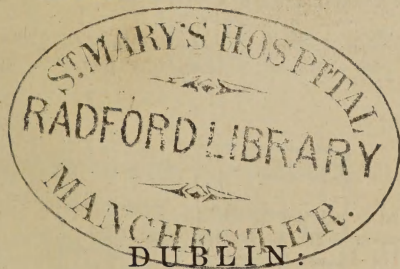
DUBLIN JOURNAL

OF

MEDICAL SCIENCE.

VOL. LIII.

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THE DUBLIN JOURNAL

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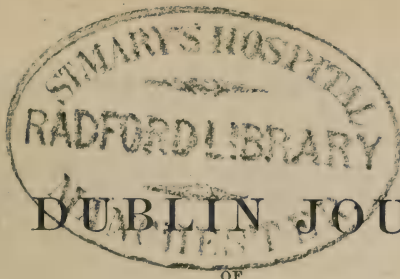
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THE DUBLIN JOURNAL OF MEDICAL SCIENCE.

JANUARY 1, 1872.

PART I. ORIGINAL COMMUNICATIONS.

ART. I.—*On the use of Phosphorus in certain Diseases of the Skin.* By HENRY EAMES, M.D., Dub.; Physician to Mercer's Hospital; Joint Lecturer on Practice of Medicine and Pathology, Ledwich School, &c.

FOR some time past my attention has been directed to the curative effect of phosphorus administered in certain diseases of the skin. I have used it in a considerable number of cases, and some of these I thought it might be of interest to bring under the notice of the society.

The metalloïd phosphorus has rarely been medicinally used in this country. Dr. Burgess, so far as I know, was the first to recommend its administration in cutaneous disease in England. He was led to do so from observation of its powerfully stimulating effects when given in the collapse stage of cholera. He employed it in lupus, psoriasis, and other inveterate skin diseases, and obtained most successful results. Recently, Dr. Broadbent read a paper before the Clinical Society of London, detailing certain cases in which he had given the drug with good effect. He, indeed, claims no special advantages for phosphorus over arsenic. He desires not to bring forward a new medicine for diseases of the skin, but only to exhibit one more analogy between arsenic and phosphorus, which, included in the same chemical group, should also be therapeutically united.

Dr. Tilbury Fox, in his excellent work on diseases of the skin, briefly recommends its use in acne, pemphigus, and pruritus. In Dr. Erasmus Wilson's classical works on cutaneous medicine, phosphorus is not once mentioned as a remedial agent.

Now, it is claimed for phosphorus that it is something more than a nauseous substitute for arsenic; and I hope to prove that phosphorus will succeed in a most marked and brilliant manner in certain cases in which arsenic has been fully tried without any perceptible curative effect. The mode of administration adopted was a solution of the metalloid in oil. Ten grains of pure phosphorus were dissolved in one ounce of olive oil, the dose of the solution being from five to ten minims three times a day after meals. It may also be dissolved in ether, or given in a solid form in pills. The opinion of Pereira, however, is opposed to this latter mode. Some have objected to the use of the oily and ethereal solutions on account of their extremely disagreeable taste and smell. Both these difficulties have been obviated by having the oily solution enclosed in membranous capsules, which may be procured from Messrs. J. J. Graham and Co., Westmoreland-street. As it is desirable to vary the dose in certain cases, there have been made three varieties of capsules, containing respectively $\frac{1}{10}$ th, $\frac{1}{20}$ th, $\frac{1}{30}$ th of a grain of pure phosphorus, and numbered respectively Nos. 1, 2, 3. For a child of three years old, No. 3 would be a suitable dose. I have found that the medicine, when given enclosed in these capsules, is much less liable to produce dyspeptic derangements, and that disagreeable eructations are less frequent. I shall now proceed to detail some cases:—

Mrs. X. came under my care on the 1st of March, 1870. She was married, and had one child. When three veils which she wore were removed there was little difficulty in arriving at the conclusion that she was suffering from acne indurata of a severe type. The eruption was abundant on her chin, which was much swollen and disfigured; also on her cheeks, nose, and forehead, and in an aggravated degree in lines leading from the inner canthus of either eye to the commissures of the lips. Four years ago the disease manifested itself by a few spots which appeared on the chin. At this time Mrs. X. was out of health, and as her general condition improved these spots disappeared. Some induration and duskiness of the integument remained, and the malady broke out again some months afterwards. This attack was also slight. After some similar attacks the disease appeared with extreme violence in

November, 1869. She was under treatment from this date till I saw her on the 1st of March. She had become continuously worse, the eruption advancing from the chin to the parts I have mentioned. She felt weak, depressed, easily fatigued. Her tongue was coated; she had nausea in the morning. Her bowels were regular, and her pulse normal; catamenia regular, but rather excessive latterly. She has taken various acids, iron, and arsenic, all without benefit. The spots have been opened, and have been touched with nitrate of silver. I ordered tincture of the perchloride of iron in infusion of calumba, and a lotion of acetate of lead and opium to be applied warm to the face, as there was great irritation present. Nitro-muriatic acid and pepsine wine were afterwards given, and an ointment containing hypochloride of sulphur was rubbed into the face. She improved slightly under this treatment; but in the beginning of April she became as bad as before. I now determined to try phosphorus, as there existed considerable nervous depression. It was given dissolved in oil; at the same time she continued to use the hypochloride of sulphur ointment. On April the 29th she was so much improved that she came without a veil. She also felt much stronger; her appetite was excellent, and she took exercise without excessive fatigue. The phosphorus was taken for six weeks, and she has since remained exempt from the disease.

The next three cases, which I shall briefly detail, are of special interest, as the malady treated was lupus, the intractable nature of which we are all acquainted with.

Theresa E., unmarried, aged 34, was attacked by the disease when she was ten years of age. It commenced by a reddish pimple, which formed at the junction of the left ala of the nose with the cheek. The malady spread continuously from this point, notwithstanding the various forms of treatment adopted in many of the hospitals of this city. The topical applications used were nitric acid, nitrate of silver, chloride of zinc, &c. Internally she took arsenic and cod liver oil in quantities, and two courses of mercury, during which she lost her teeth. The disease had advanced over both her cheeks, had completely eroded the cartilages of the nose, had spread down both sides of her neck and under her chin; it had also affected her left wrist, and a considerable part of her left forearm. She had not been under treatment for a considerable time, when a renewed assault of the disease, which began to spread rapidly on her forehead, induced her to present herself at our dispensary.

This seemed to be a most unpromising case—all ordinary treatment had been exhausted. It was determined to test the effect of phosphorus. After a fortnight there was a visible improvement in her condition, the parts lately affected being healed. The disease was driven back. For nine months she took ten minim doses of the phosphorated oil three times a day. It did not purge her; nor did it produce diaphoresis. She passes rather more water than usual. Her menses, which had been irregular and scanty, have become normal in time and amount. The disease became limited to the left cheek, on the part where it commenced, to a patch about the size of a crown piece. The wrist and arm are perfectly healed.

At this time she began to suffer from severe dyspeptic symptoms. Her tongue was coated with a translucent white fur, and she had continually eructations tasting of lucifer matches, and there was considerable nervous depression. Under these circumstances it was thought necessary to cease the administration of the drug, and mineral acids and quinine were substituted for it. After a month the phosphorated oil was resumed, being now combined with cod liver oil. She took these remedies for three months continuously, when symptoms similar to those occurring previously exhibited themselves. She was now admitted into the hospital, as it was thought that she was not sufficiently nourished at home. A tonic treatment was at first adopted, and after a fortnight, the phosphorated oil was resumed. She has since then taken the drug continuously, with the exception of some short intervals, during which it was deemed right to substitute a tonic treatment. She is now nearly cured, a small spot only remaining in the situation where the disease commenced.

Mary K., married, aged 53, first seen at the dispensary, February, 1870. The disease began 26 years ago, as a small, brownish, yellow scab, on her left cheek, near the ear. Starting from this point, the disease spread continuously across her face, sparing the forehead and not injuring the cartilages of the nose. It involved also her neck on both sides, and her chin. The remedies, internal and external, which had been used in her case would furnish quite a long list. Suffice it to say, that arsenic, mercury, and cod liver oil had been abundantly tested. When she presented herself her face and neck were covered with incrustations, beneath which was a thin purulent fluid. Opening her mouth was so painful, from the soreness of her jaws and the angle of her lips, that she took food with the greatest difficulty. The disease was evidently advancing

at the edges, and had cicatrized in but few parts. She was ordered phosphorated oil in five minim doses, gradually increased to ten minims, three times in the day. Its beneficial effects were manifested almost from the commencement. She took it continuously for five months, at the end of which time all ulcerations had disappeared, and cicatricial appearance of the skin alone remained. The malady has not recurred during eighteen months, so that the cure is probably permanent, and one could tell only by close inspection that she had ever been the subject of lupus non-exedens.

Kate C., a domestic servant, aged 33, unmarried. When first seen she had on her left cheek a patch of erythematous lupus fully as large as a crown piece. It was of a dusky red colour, and presented the appearance as if currant jelly had been spread on the part. She had some aching pain in the part, and it was painful to the touch. The edges were slightly elevated above the surrounding skin. The disease commenced when she was 16 years of age, and had gradually increased to its present size. She had taken arsenic and cod liver oil, and had used various topical remedies without good effect. Her general health was good, with the exception of her menses, which were scanty and accompanied with pain.

She was ordered ten minim doses of the phosphorated oil, and also to apply the oil to the diseased part. This latter produced a greyish, sloughy condition, and was discontinued, simple ointment with creosote being substituted for it. This was stopped after a fortnight, and zinc ointment was used for the remainder of the time she was under treatment. She took the phosphorated oil for nine weeks with great advantage, the diseased part being reduced to the size of a florin, and her periods having become perfectly regular and free from pain. Grave dyspeptic symptoms necessitated the discontinuance of the drug, and acids were given for a fortnight. It was then resumed, and taken for six weeks more, when the part was entirely healed, all irritation being absent. I have seen her lately; she continues well, having had no return of the disease, and but a very slight mark remaining where the sore had been.

Scrofulo-derma is one of the diseases in which phosphorus exhibits its most beneficial action. The following cases are examples of this:—

Michael Runkerty, aged 9 years. Has been ill for three years with swollen glands. The disease exhibited itself first in his

right arm, and afterwards in his neck. When brought to the dispensary he had seven swellings on his arm, two in his axilla, and four on his neck. The skin over these spots was of a dusky red colour, and thickened. He had taken for a long time iron and cod liver oil. He was ordered five minim doses of the phosphorated oil three times a day. In six weeks the swellings had disappeared, and the skin was of its natural texture and appearance.

Richard O'Malley, aged 27, unmarried. Had been under treatment for pulmonary consumption about a year ago; was attacked three weeks since with swelling in his left axilla. The skin in this part is of a dusky red colour, and thickened. He was given phosphorated oil in ten minim doses three times a day for three weeks, and at the end of that time presented himself cured of the disorder, all traces of the swelling and of the discolouration of the skin being removed.

In the treatment of psoriasis I have found phosphorus very efficacious, even in cases in which arsenic had proved of no service. The following cases are examples of this:—

Mr. W., aged about 40, who had served in India, became, whilst there, affected with psoriasis, which appeared on his legs first, then on his chest and back. This was some two years before he came under my care. He had taken arsenic for some time without good effects. I determined, however, to try the ferso arsenical mixture of Dr. E. Wilson, applying the glycerine of carbolic acid externally. This was continued for three weeks. His condition under this treatment became rather worse. He was now put on phosphorated oil, the external application being the same as before. In five weeks he was perfectly free from the eruption.

James Farrell, aged 20, a labourer, of a fine healthy appearance; never had syphilis. Parents both alive, healthy, and temperate. He was admitted into Mercer's Hospital on September 12th, 1871, suffering from severe psoriasis. He was first attacked 14 months previously. He had during this time been under the able treatment of four medical gentlemen. The disease began as psoriasis guttata on his chest and abdomen. When I examined him I found him almost covered with the eruption from the crown of his head to his feet, the disease being markedly of the diffused variety. He had taken mercury, the perchloride as well as some other salts, iodide of potassium, and, as he affirmed, about a pint of Fowler's solution.

Externally he had used various applications, including the tarry preparations, and carbolic acid. He was at once ordered phosphorus, to take a warm bath every second night, and to rub in carbolated oil. After a fortnight dyspeptic symptoms occurred which compelled us to discontinue the drug for a week, mineral acids being given. The phosphorus was then resumed, and the only change made in his treatment was giving him a hot air bath (Wyatt's) three times a week. He left the hospital on Oct. 21st almost well. I have since heard that he is quite cured.

Mr. L., aged about 30, married, holding an official position in India, placed himself under my care on November 2nd, 1871. Had never had syphilis. His general health was excellent; but there was a history of gout in the family. He first became affected with the disease about three years ago, whilst in the Madras Presidency. It commenced upon his right shin, and has gradually spread, so that when I first saw him the psoritic patches were distributed over his entire body, both in front and behind, over his hands and arms, legs and feet, as well as over his forehead, and scalp. He had returned from India a year previously on furlough, and had at once placed himself under medical care. I saw some of the prescriptions for the drugs that he had taken. Both mercury and arsenic had been amply tested. Despite their use, and during it, the malady had become progressively worse. I ordered hot air baths, glycerine of carbolic acid to be rubbed into the affected parts, and to take No. 3 capsule twice a day. After pursuing this treatment for ten days, he found that glycerine of carbolic acid was too stimulating, and one of the tarry preparations was substituted, and No. 2 capsule instead of No. 3. After ten days more, No. 1 was given; and now, December 17th, the disease is cured, patches of discolouration only remaining, where the eruption had been seated.

The following case of pemphigus exhibits in a marked degree the power of phosphorus as a curative agent:—

Edward H., a domestic servant, aged 24, admitted into Mercer's Hospital, August 3rd, 1870. Unmarried; never had syphilis. He had been out of health, but without any definite ailment, some fourteen months previously. At this time an eruption consisting of vesicles in groups broke out on his abdomen. The vesicles varied in size from a split pea to a hazel nut. The eruption soon afterwards appeared on his chest and back, and had shortly

before his admission to hospital spread down his arms and legs. He had taken various tonic remedies and large quantities of arsenic, and had also used ointments and lotions. He was ordered five minim doses of the phosphorated oil three times a day, and baths of bran and warm water. The drug produced no ill effects on the digestive organs, and he continued to take it in increasing doses for nearly one month, when the dose was gradually diminished, and in one week more he was discharged cured.

I have found that phosphorus produces a coated state of the tongue, not unlike the silvery tongue which follows the prolonged use of arsenic. Loss of appetite, mental depression, and bodily weakness also are induced much earlier in some cases than in others, but to be watched for in all cases in which the drug is given. On the earliest appearance of dyspeptic symptoms I now discontinue its use, and administer some of the mineral acids. Many patients have mentioned that some two or three minutes after taking the medicine a pleasant sensation of warmth is felt through the entire body. I have not ascertained that any aphrodisiac effect is produced, though I have frequently inquired after it. A slight diaphoresis is observed occasionally. Diarrhœa was not induced in any of my cases. The amount of urine in some was slightly increased. An analysis made by my friend Dr. Cameron failed to detect any deviation from the standard of health.

Most of the foregoing cases had been treated by arsenic and other drugs before coming under my care. I have at hand notes of numerous other cases, which have been from the first treated successfully with phosphorated oil: these I have not detailed, as the intention of the present paper is to prove that the administration of phosphorus will cure certain cases of cutaneous disease, even after mercury, arsenic, and other remedies, vaunted as specifics, have completely failed to do so. I may mention, however, that eczema of the scalp, with enlargement of the glands of the neck, is very amenable to treatment by phosphorus. The strumous character of this disorder and the difficulty of its removal are very generally admitted. I have seen in many cases a copious eczematous eruption on the head and behind the ears, combined greatly enlarged *glandulæ concatenatæ*, disappear in a few weeks with after this treatment had been adopted, the glands being also quickly restored to their normal size.

It has not fallen within the scope of this paper to speak of the good effects of phosphorus in certain neurotic diseases in which I

have found it act beneficially. I trust, however, that sufficient has been said to bear out the statement that phosphorus is entitled, as a curative agent, to the earnest attention of the dermatologist.

ART. II.—*Some Notes on the Treatment of Small-pox.* By WILLIAM STOKES, M.D., D.C.L.; Regius Professor of Physic in the University of Dublin.

IN drawing attention to the subject of small-pox, I propose to dwell on one or two points of practice, rather than enter on any disquisitions as to the nature and preventibility of the disease. It was supposed by some that small-pox had, to use a modern and not very intelligible phrase, been stamped out in Ireland by vaccination. Those who thought so seem to have forgotten that a large portion of the adult population in the country had never had the advantage of the system of dispensary vaccination, while also that inoculation was covertly practised, and I well remember the warning long since given by Sir Dominic Corrigan, that the benefits of vaccination under the Poor Law Dispensary system, could only be hoped for among those born after the passing of the Act. The existence of the present epidemic shows the wisdom of Dr. Burke's observation when examined before the Royal Sanitary Commission. On being asked his opinion as to the preventive system adopted under the Poor Law, he said that it must be tested by the occurrence of an epidemic.

I may allude here to a short paper which I published in the *Dublin Quarterly Journal* for May, 1860, on the Prevention of the Pitting of the Face in Confluent Small-pox. In this paper I endeavoured to show that, with reference to the probability of pitting, we must take largely into account the character of the disease. If we look at two cases of confluent small-pox, in which the amount of eruption and of confluence are equal, or nearly so, but yet with opposite characters of fever, the chance of pitting seems to be much greater in the sthenic than in the asthenic or typhoid case. From the rarity of the highly inflammatory or sthenic variola during the last quarter of a century, we now rarely see a case of marked pitting. Whether this has arisen from the general change of type of disease, or the influence of vaccination, is a question. Yet it is unlikely that the modifying effect of vaccination in the individual could alter the type of epidemic disease. I believe there are few affections that better illustrate

the change in the character of disease, which seemed to follow the invasion of cholera, than that of small-pox. After that time most of our hospital cases have been more or less of the asthenic type. There was extreme weakness, often a livid hue of the skin, a tardy and irregular advance of the pustules, and an early tendency to confluence; the pulse rapid and compressible, and the action of the heart feeble, but jerking. In fatal cases, at about the middle stage of the disease, many of the pustules on the legs and feet took on a deep purplish or livid hue, their appearance approaching that of the vesicles in gangrenous erysipelas.

Another distinction between the two types of the disease was seen in the greater or less amount of cutaneous vascularity, heat, and swelling which attended the eruption, especially on the face. In the sthenic type these conditions were intensely developed.

Looking at the frequency of the occurrence of pitting on the face as compared with that of other parts of the surface, may it not in part be accounted for by the fact that while the rest of the body is kept covered by the bed-clothes, and in a state of comparative humidity, the face remains in a dry and heated condition from the influence of the external air, and the increased vascular action. This we have seen sometimes so intense that no oily application could be used without immediately drying on the surface. Hard and hot scabs were formed, and the suppurative process made its way downwards to a greater or less degree.

It was in the year 1849 that I saw the most severely inflammatory or sthenic case that I have had to deal with before or since. There was great tumefaction, accompanied with extraordinary heat of the face, and, in the hope of saving the eyes, poultices were applied over them. The patient recovered, but with deep and permanent pitting; but at the time when the crusts were formed with their usual dark colour, the countenance presented an extraordinary appearance, the integument of each orbital region being free and almost white, while the rest of the face was everywhere covered with deep black crusts. There was no pitting on the eyelids, or in their immediate neighbourhood. From the date of this case, I have adopted as a routine practice the application of light poultices over the entire face, or of a mask of lint steeped in glycerine and water and covered with a corresponding mask of oil-silk, and have found, but with one exception, that pitting was effectually prevented. In that case the patient was delirious, and could not be prevented from tearing the poultices off his face. I believe that if from an

early period we protect the surface from the air, and keep it in a permanently moist condition, marking will seldom occur.

Experience entitles us to hold that, other things being equal, the tendency to pitting, or, in other words, the virulence of the pustulation, is directly as cutaneous vascularity and heat.

This method fulfils three important indications of treatment—

1st, The exclusion of air.

2nd, The keeping of the parts in a permanently moist state so as to prevent the hardening of the scabs.

3rd, The lessening of the local irritation.

I need not remark that the value of the treatment is best seen in the inflammatory or sthenic type of the disease, although in the asthenic or typhoid form the occurrence of marking may be observed particularly in the confluent cases.

In reference to the importance of lessening the local irritation, the following case, to which I have alluded in my paper, is interesting:—A strong and healthy young woman was admitted with symptoms of an early stage of the ordinary epidemic fever, but of a very active type. The skin was hot and dry, the pulse strong and full, and the tongue loaded. She complained of intense headache, much more severe than that commonly observed in the commencement of ordinary fever, so that I thought it necessary to apply leeches freely to the temples. The headache, heat of the head, and flushing of the face were completely relieved in consequence. Within the next two days small-pox vesicles appeared on the neck and bosom. The case proved a most severe example of confluent variola over the trunk and limbs, while on the face not more than two or three small aborting pustules made their appearance.

Who can doubt that in this instance the depletion of the face influenced the local progress of the disease?

This remarkable case, together with our knowledge of the good effect of poulticing in lessening the irritation, and the consequent pustulation of the part, has led me, even after the vesicles have appeared, to apply small numbers of leeches once or twice to the inflamed skin, and with excellent results. The heat, vascularity, and swelling were much diminished and the pustulation clearly modified. We have here, then, a further evidence *that the activity of pustulation is directly as the inflammatory state of the surface.*

It may be observed, parenthetically, that the influence of local depletion on the development in the part of the secondary lesions of essential disease, may explain the theory, and, in some

degree, excuse the treatment of fever, by the so-called physiological school. The benefit of leeches to the epigastrium and ileo-cæcal regions, so strongly appealed to in defence of the theory, may have been that it lessened the preceding irritation which favoured the follicular engorgement and consequent ulceration of the intestine.

In the paper in the *Quarterly Journal*, already alluded to, a case is given which had been commented on by Dr. Graves. A man was for some time under treatment in the surgical ward for a chronic affection of the knee-joint. I cannot say whether it was or was not rheumatic in character, but it was treated by strapping with mercurial plaster. After some time he showed symptoms of fever, soon followed by the appearance of variolous vesicles. He was transferred to the medical wards, and went through an attack of severe and confluent small-pox. At the period of the *stadium decrustationis* of Hebra the plaster fell off, when the whole region which had been covered by the straps was found smooth and of a glistening whiteness, contrasting strangely with the black and rugged surface of the limb above and below the joint.

In explaining this I incline much more to the effect of the pressure on the cutaneous capillaries than to any specific action of the mercury.*

I have come to this conclusion, that in all cases, whether of the asthenic or sthenic form, the local treatment of poulticing is superior to any other. Two great conditions are by it fulfilled—one the lessening of the cutaneous irritation, the other the prevention of the drying of the scabs. It will be obviously more applicable in proportion to the inflammatory state of the surface.

In one case thus treated the result was satisfactory so far as the prevention of pitting over a large portion of the face was concerned, but the tardiness in the falling off of the scabs on the cheeks was remarkable; the character of this case was asthenic or typhoid. The eruption came out slowly, and was not well formed until after stimulants had been used. These, too, had to be employed liberally and for a long period of time. Many of the scabs remained strongly adherent for two months after the first invasion of the disease. As they fell off, however, the skin was found to be uninjured. When portions of the loosening scab were detached, long filamentous processes, very similar to *asbestos*, were discovered running from the under surface of the scab into the skin. At the

* The *Emplastrum de Vigo*, formerly used to prevent pitting, was a compound into which mercury entered.

date of this patient's discharge from hospital a few of the scabs were even still adherent. Could these filamentous processes have resulted from a pathological transformation of the purulent matter?

We have seen that the treatment by poulticing or other moist applications may be said to owe its success, 1st, to the diminution of the cutaneous irritation; 2nd, to the keeping of the pustules moist; and, 3rd, to the exclusion of air. This leads us to the treatment by the warm bath practised by Hebra, who seems to have been directed to it by observing its efficacy in the management of burns. It is clear that in the case of the continued warm bath we have the conditions just mentioned completely fulfilled, and that, too, as regards the entire person of the patient.

Not many years since one of our students, a very large and robust man, was attacked with small-pox, which soon showed itself in its worst characters. The fever at first was very high, and the head swelling and vascularity of the face intense. The *eruption was universal*, while the pustules on the face became confluent at an early period. Delirium set in, and the patient tore off the dressings from his face so often that we desisted from their further application. After the tenth day the condition of the patient was most appalling. The delirium continued, the circulation became every day weaker and more rapid, notwithstanding the free use of stimulants; the crusts were not only black, but on the legs, where here and there there was less confluence, the blackness of the worst purpura appeared—a condition held by Hebra to be always fatal. The body was one universal ulcerous sore, and the agonies of the patient from the adhesion of the surface to the bed-clothes were not to be described. In addition to the usual fœtor of small-pox in the stage of decrustation which was present in the highest degree, there was an odour of a still more intensely pungent and offensive character, which seemed to pass through the by-stander like a sword. I never before or since experienced anything similar. Stimulants alone, freely and constantly employed, seemed to preserve the patient alive; the pulse was rapid, weak, and intermitting, and for several days we despaired of his life.

At this juncture I happened to describe the case to my colleague, Mr. Smyly, who suggested the trial of the warm bath, with the view of relieving the terrible suffering. A bath in which he could recline was speedily procured, and pillows being adjusted in it, we lifted the sufferer in and placed him in the recumbent position. The effect was instantaneous and marvellous

The delirium ceased as if by magic; it was the delirium of pain, and the patient exclaimed, "Thank God! thank God! I am in Heaven! I am in Heaven! why didn't you do this before?" The fœtor immediately and completely disappeared, so that on entering the ward no one could suppose that there was a case of small-pox in it. He was kept at least seven hours in the bath, during which time brandy was freely administered, and omitted only when he showed symptoms of its disagreeing with the brain. He was then removed to bed. The surface was clean, and in many places the sores looked healthy and white. The bath was repeated next day, after which he fell, for the first time, into a tranquil slumber. From this time his recovery was progressive, delayed only by the formation of abscesses and the great soreness of the feet.

That this gentleman's life would have been sacrificed but for the timely use of the bath, few who have had any experience in prognosis can reasonably doubt. He was in the condition of a patient every portion of whose skin had been burnt and ulcerated. The pustulation was almost universally confluent; the purulent matter highly putrescent; the hæmorrhagic state developed, and the nervous system suffering; in fact, he had every symptom of the worst putrid absorption.

This case and its singular result, in addition to the experience of Hebra, justifies the recommendation of the use of the bath. No danger attends its employment, and, in asthenic cases, stimulants can be freely used. In the Vienna hospital patients have been kept continuously in the bath for one hundred hours with good effect.

I crave your forgiveness for the length of this paper. The last case speaks for itself; its character has been more under than overdrawn, and we cannot doubt that the mortality in small-pox hospitals would be greatly lessened by the use of the bath. My colleague, Dr. Foot, joins with me in the use of it in our wards. Even an ordinary hip bath, in which the patient sits on a blanket, the ends of which are made to cover his shoulders and legs, is most grateful and efficacious.

I must not conclude without expressing my admiration for the devotion of Dr. Darby, of Monasterevan,—then Demonstrator of Anatomy in the Cecilia-street School—to his pupil, when in this loathsome and terrible condition. For more than a fortnight he stood by him day and night, without one hour's intermission, performing all the duties of nurse, comforter, and physician. His intrepidity and his noble charity met their reward.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

DISEASES OF CHILDREN.

1. *A Practical Treatise on the Diseases of Children.* By ALFRED VOGEL, M.D., Professor of Clinical Medicine in the University of Dorpat, Russia. Translated and edited by H. RAPHAEL, M.D., from the fourth German Edition. New York: Appleton and Co. Pp. 603. 1870.
2. *A Practical Treatise on the Diseases of Children.* By J. FORSYTH MEIGS, M.D., Physician to the Pennsylvania Hospital; Consulting Physician to the Children's Hospital, etc., etc.; and WILLIAM PEPPER, M.D., Physician to the Philadelphia Hospital, etc., etc. Fourth Edition. Philadelphia: Lindsay and Blakiston. Pp. 921. 1870.
3. *The Diseases of Children.* By FLEETWOOD CHURCHILL, M.D., Dub. and Edin., Fellow and Ex-President K. & Q.C.P.I., etc.; and FLEETWOOD CHURCHILL, Jun., F.K.Q.C.P.I., L.R.C.S.I.; Physician to the Dispensary for Sick Children, etc. Third Edition. Dublin: Fannin. Pp. 900. 1870.
4. *A Practical Manual of the Diseases of Children, with a Formulary.* By EDWARD ELLIS, M.D., Physician to the Victoria Hospital for Sick Children, etc., etc. London: Churchill. Pp. 279. 1869.
5. *Handy-Book of the Treatment of Women's and Children's Diseases, according to the Vienna Medical School, with Prescriptions.* By Dr. EMIL DILLNBERGER. Translated by PATRICK NICOL, M.B. London: Churchill. Pp. 208. 1871.
6. *Scarlet Fever, a Manual for Mothers and Nurses.* By JOHN MARSHALL, M.R.C.S., L.R.C.P., etc. London: Churchill. Pp. 36. 1869.

7. *Hooping-Cough; its Pathology and Treatment.* By PATRICK MARTYN, M.D., Lond., B.L., Surgeon in Her Majesty's Fleet. London: Churchill. Pp. 60. 1869.
8. *On the Causes, Prevention, and Treatment of Infantile Diseases; showing by what means the present Mortality may be greatly reduced.* By SELBY NORTON, M.D., etc. London: Churchill. Pp. 75. 1870.
9. *The Treatment of Croup.* By FORDYCE BARKER, M.D., etc. Reprinted from the *American Journal of Obstetrics and Diseases of Women and Children*, May, 1870; with remarks by one of the Editors, A. JACOBI, M.D., Clinical Professor of Diseases of Children in the College of Physicians and Surgeons, New York. New York: Townsend and Adams. Pamphlet, pp. 16.

THE great progress which the study of infantile diseases has recently made, is remarkably shown by the number of works which have lately made their appearance on the subject not only at home, but in France, Germany, and America. Still we must express our hope that these labours will only be an incentive to the further study of a most important class of cases but too notably neglected, and from the almost total absence of any clinical teaching on which, students are sent out to practice on obtaining their diplomas, with, but at the most, a very superficial knowledge of the diseases of a large per-centage of those they will be called on to treat. Foreign countries have been much in advance of us in the establishment of hospitals for sick children. Until within the last nineteen years, even in London, no children's hospital proper, existed; but now several have sprung up there and in the large towns of England and Scotland, and, by the increased and accurate knowledge only to be attained by the study of a large number of cases collected together, they attest the special value of such institutions. In Dublin, unfortunately, the opportunities for the clinical study of infantile diseases are very limited. There is no special hospital^a for the systematic instruction of students in this essential part of their medical education, and in three only, we believe, of the general hospitals are there children's wards, the majority of

^a The Pitt-street Institution, although a most excellent charity, and largely attended, does not put up any beds.

cases in which are generally surgical. Students, therefore, must depend—we are speaking of medical cases—upon a chance little patient in the wards, or whatever they may vaguely acquire in their attendance at a dispensary, or from books. Any practical acquaintance, therefore, under such circumstances, with the maladies of children, by all acknowledged to be a subject surrounded with obstacles, must be most difficult to obtain, even by a diligent student, and a knowledge of pathological anatomy from an autopsy, almost impossible.

The work of Dr. Churchill, which has now reached its third edition—and has had the advantage of revision by his son, Dr. Fleetwood Churchill, jun.—must be so well known to the readers of the Journal, that any further notice, beyond mentioning its appearance, is unnecessary.

The treatises of Dr. Vogel, and of Drs. Meigs and Pepper, have been long received, and would have been noticed previously had we had an opportunity for giving them the full consideration we desired, and their importance amply deserved.

The merits of Dr. Vogel's work—which has been translated into three languages, and has attained its fourth edition in less than eight years—are of a high order; the pathology especially is most complete, the deviations peculiar to infantile life being fully explained, and an amount of knowledge and anatomical research displayed, which could only have been obtained by opportunities afforded for the accurate study of a large number of cases; indeed the entire work bears evidence of intelligent observation and original thought. Diagnosis and therapeutics occupy a less important part; little faith, as a general rule, is put in medicine, the remedies employed being few in number, but always given with clearness and a definite object; the important subjects, however, of prophylaxis and of hygienic and dietetic treatment are fully dwelt upon. The translation has been ably performed by Dr. Raphael, of New York, and is illustrated by six lithographic plates.

The work of Drs. Meigs and Pepper is also a fourth edition of a popular American publication. We have no hesitation in saying that it is a treatise of extreme value and of great practical utility, the authors have indubitably had large experience, and are fully *en rapport* with the present state of knowledge. As a text-book and general guide to practitioners, as well as a standard work of

reference, we know of no other volume which contains more useful or reliable information.

Dr. Ellis's manual is a much more unpretending work than any of the former; it gives a succinct and, for the most part, compiled account of the prominent diseases of children, written in rather a loose but yet practical manner. Such statements, however, as that—"The normal temperature of the child, taken, say under the armpit, is 88° to 98° F." And that intestinal hemorrhage in typhoid of children is common, detract much from its value. A main feature of the book is an extensive formulary, occupying 60 pages, in which the drugs are arranged without any definite order under such headings as "blood-restorers" (iron, manganese, and cod-liver oil), "antacids," "astringents," "acids," &c., each group being followed by numerous prescriptions of the remedies comprised therein, in different combinations. This portion of Dr. Ellis's work; and likewise Mr. Nicoll's translation of Dr. Emil Dillnberger's *Handy-book of the Treatment of Women's and Children's Diseases*, which also consists in a copious collection of prescriptions, and suggestions for treatment according to the Vienna Medical School, may be useful to those who are fond of polypharmacy, or require assistance in the selection of a remedy, or the usual mode of prescribing one.

Mr. Marshall's little work is one of useful suggestions for those in attendance upon scarlet-fever patients, comprising the modes of isolation, ventilation of the sick-room, and disinfection of the excretions, &c. As the manual is intended especially for mothers and nurses, acting in every case under advice, no mention is wisely made regarding any medical treatment.

Dr. Patrick Martyn, Surgeon in Her Majesty's Fleet, and who is also, he informs us, the "discoverer of the function of the thyroid body or vocal brace (colligator vocalis), &c., &c.," has, in addition, made the wonderful discovery of the "one new and true" theory of the pathology of hooping-cough. The disease is, according to him, "an inflammation of the upper portion of the trachea." In one place he says the hoop and cough are "both the production of the glottis;" in another, that the vibration of the chordæ vocales produces the hoop. He gives us no proof, pathological or clinical, of the accuracy of his views, or results of his "successful treatment"

(an antimonial saline mixture with chloric ether), or the duration of his cases, or prevention of complications. His "essay," which he modestly informs us is "somewhat discursive" (very much so), concludes with a chapter on prevention, applicable as well "to all other diseases of the lungs,"—having previously stated that hooping-cough was neither a catarrhal, bronchial, or pulmonary affection—from which we are tempted to make the following choice extracts; commenting on the frequent insufficiency of children's clothing, he says:—

"But the poor children, if fashion exact the nakedness of parts of their bodies, then let it be some part that can bear the exposure with impunity, as the part that gets the whippings."

And recommends as the best exercise for girls:—

"Dancing, that girls are so fond of, that rouses the languid circulation and prevents that sapping infirmity of young girls, cold feet; that conduces to dignity of mien and grace of movement; that cultivates a musical taste, and enforces a correct sense of time. Often in foreign parts, when witnessing heathen modes of prayer, wound up with a dance, did I wish that it could in some way be introduced at home after night prayers, so as to send the young girls to bed with warm feet."

Certainly we might apply with justice Dr. Martyn's *critique* on others to himself, for what *he* has "written on hooping-cough is vague and impracticable," and its composition is "as unsatisfactory as its pathology." *Mutato nomine de te fabula narratur.*

We will notice Dr. Norton's "Infantile Diseases;" and Dr. Barker's important therapeutical observations, in the course of the following *precis* of the opinions of the authors of the first two books on our list, as to the nature, pathology, and treatment of some of the most frequent diseases of children.

That diarrhœa is, in a great majority of cases, dependent upon improper food, is a well-known fact; and as a preventable cause of a disease occasioning the largest portion of infantile mortality, the question of diet is worthy of the most minute attention. Breast-milk is alone the proper food for infants, but when, from various circumstances, it is compulsory to bring up the nursling by artificial feeding, cow's milk is the best and most practicable substitute. The mode of its preparation, however, is important: for very young children, Vogel recommends that the

milk should always be rendered alkaline by adding to the quantity taken at each meal a teaspoonful of a solution of carbonate of soda (3i. to water 3vi.), diluting it one-third with water, and sweetening with milk-sugar. Drs. Meigs and Pepper prefer giving two parts of water to one of milk of full average richness, sweetened always with milk-sugar (3ij. to each 3viii.), increasing the proportion of milk, after the child is a month or two old, with care. They are not in the habit of regularly using lime-water instead of simple water as a diluent, unless when the milk is rejected curdled, or the stools contain lumps of undigested caseine. The proportion they recommend is from an ounce to an ounce and a half of lime-water to each pint of food. Some children, especially those suffering from indigestion, digest cow's milk more readily when it is diluted with three, four, or even five parts of water; cream, equally diluted, in other cases agrees better. As to the quantity to be given in the twenty-four hours, Drs. Meigs and Pepper, from the results obtained by a comparison between the quantity of milk secreted by nursing women, and calculations based upon Dr. Parkes' estimates as to the amount of food necessary for adults, say, that "in the first ten days after birth a pint or a little over, is about the right quantity." After that period a pint and a half to a quart up to the second month. No other food but milk thus prepared should be given for the first three months according to Vogel; and Dr. Norton's little book, under its most misleading title of "*Infantile Diseases*," is nothing more than a forcible protest against the use of that "poisonous agent starch" which alone, in his opinion, causes "at least 90 per cent." of the deaths in children under one year of age; but in noticing the different vaunted and complicated preparation for infant's food, the American authors, while strongly of opinion that amylaceous substances during the early months are of no value as compared with a sufficient supply of good milk, still, "have found by experience, that a small quantity of amylaceous material combined with the milk does render the food more digestible;" and suggest, in explanation, that the particles of starch, by being interposed between the elements of the casein, lessen the tendency of cow's-milk to coagulate in hard large masses—a fact which Vogel states constitutes the only essential difference between it and woman's-milk; the latter always coagulating into small lumps and loose masses. They recommend as the best substitute, as a rule, for the "natural aliment," the following preparation:—

“It is made by dissolving a small quantity of prepared gelatine or Russian isinglass in water, to which is added milk, cream, and a little arrowroot, or any other farinaceous substance that may be preferred. The mode of preparation and the proportions are as follows:—A scruple of gelatine (or a piece two inches square of the flat cake in which it is sold) is soaked for a short time in cold water, and then boiled in half a pint of water until it dissolves,—about ten or fifteen minutes. To this is added with constant stirring, and just at the termination of the boiling, the milk and arrowroot, the latter being previously mixed into a paste with a little cold water. After the addition of the milk and arrowroot, and just before the removal from the fire, the cream is poured in, and a moderate quantity of loaf sugar added. The proportions of milk, cream, and arrowroot must depend on the age and digestive power of the child. For a healthy infant within the month, we usually direct from three to four ounces of milk, half an ounce to an ounce of cream, and a teaspoonful of arrowroot to a half pint of water. For older children, the quantity of milk and cream should be gradually increased to a half or two-thirds milk, and from one to two ounces of cream. We seldom increase the quantity of gelatine or arrowroot.

“We have given this food to a great many children during the last twenty years, as well to those brought up entirely by hand, as those partly suckled, or weaned, and can truly state that they have thriven better upon it than upon anything else we have employed. In several cases it has agreed perfectly well with infants who could not, without vomiting, diarrhoea, and colic, take plain milk and water, cream and water, any kind of farinaceous food prepared with water, chicken water, or in fact any other food that had been tried. In the cases of sick children, it ought sometimes to be made even weaker for a while than in the proportions first mentioned above.”

Vogel considers Liebig's soup as “the most rational of all substitutes for the mother's milk;” but Meigs and Pepper have been disappointed with it.

When in the diarrhoea of children at the breast, dietetic or medicinal treatment of the nurse is not sufficient to arrest it, Vogel adopts a new and valuable mode of administering opium to the infant, from which he has never observed any bad effects. He dips a camel's-hair brush into tinct. opii., shakes off the first drop by snapping it with the finger, and then introducing it into the mouth of the child, presses the chin a little upwards, and pulls the brush out from between the compressed lips. In the intestinal catarrh of children brought up by hand, which is totally different from that of those at the breast, cow's-milk is not tolerated in any form,

and must be totally abstained from for a few days, giving instead, "thin mucilaginous beef-broth with rice, barley, or groats, slightly sweetened with sugar, but deprived of fat, and without salt. . . . The pencilling of the mouth with laudanum, and the use of opiate clysters, stand at the head of all therapeutic measures. But occasionally, in the profuse diarrhœa of summer, opium proves inefficacious; then small doses of calomel, gr. $\frac{1}{8}$ three or four times daily, or a solution of nitrate of silver (gr. ss. to water ℥iii.), with the addition of one drop of laudanum, without any syrup, proves more effectual. . . . In some instances I have seen the diarrhœa checked by a solution of alum (gr. vj. to mucilage ℥ij.), though it was not possible to arrest it by any of the remedies just mentioned. The principal treatment will always be a proper prophylaxis." Vogel also states that the essential anatomo-pathological difference between intestinal catarrh and follicular enteritis, consists in the participation of the mesenteric glands in the latter; and further remarks, in connexion with *tabes mesenterica*:—

"It is much to be regretted that neither by injections nor in any other manner can it be experimentally proven that the absorption of the chyle is hindered by these hypertrophied mesenteric glands, and thereby the nutrition and progressive development of the child interfered with. But, when an atrophied child, whose condition was originally induced by enteritis folliculosa, no changes but those indurated and hypertrophied mesenteric glands are found, the supposition becomes very probable, that the passage of the chyle has been mechanically interrupted, and thus the children, although they have consumed an enormous quantity of food, and have had no diarrhœa for weeks preceding death, have nevertheless languished to a certain extent for want of a sufficient supply of chyle."

Drs. Meigs and Pepper compare chronic entero-colitis (follicular enteritis) or inflammatory diarrhœa of children to the same disorder in armies and camps, and consider the causation, symptoms, anatomical lesions, pathology and results of treatment analogous. The seat of the disease they state to be more frequently in the large than in the small intestine, and in most cases limited to the colon. "The lesions consist of increased vascularity, thickening and softening of the mucous membrane of the intestine, and enlargement of the intestinal follicles," running on into "discolouration, thickening, with infiltration, and induration of the walls of the intestine, and more or less destruction of the mucous membrane from follicular ulceration." They are of opinion that the disease is seldom

dangerous when it occurs as a primary affection ; while, as a secondary one, it is much more apt to be serious. Vogel, on the other hand, says that "as a rule all methods of treatment are inefficacious," with the exception only, in some cases of breast-milk, "when the circumstances are such as to preclude the possibility of procuring a wet-nurse the prognosis is *"fere lethalis."* The diet constitutes the most important point in the treatment, and should be carefully regulated. The gelatine and milk preparation, noticed previously, made very weak, is a good artificial food. Drs. Meigs and Pepper, instead of depletion, use warm baths (95° to 97°) twice or thrice daily, poultices to the abdomen, or warm stupes, and refrigerant medicines. Small doses of the sulphate of magnesia and laudanum are very useful. Calomel has disappointed them, but they still put trust in the blue pill mixture, composed of bi-carbonate of soda half a drachm ; mass. hydrarg., three grains ; camphorated tincture of opium, a drachm ; simple syrup, two drachms ; mint water, to two ounces. Dose, a teaspoonful every three or four hours.

Opium is one of the most valuable remedies ; it may be given early in the case with a laxative, or by enema ; afterwards as Dover's powder. For an enema for a child at one year, two drops of laudanum in one or two teaspoonfuls of water, or thin starch water may be used two or three times a day. A small urethral tin syringe is the most suitable instrument for its administration (Vogel). Small opiate suppositories, gr. $\frac{1}{12}$ th to $\frac{1}{6}$ th, made up with cocoa-butter, are most useful, especially in the chronic form and in dysentery, from their local as well as constitutional effects. If astringents are necessary, chalk mixture, either alone or with tinct. krameriaë, or some opiate preparation, or with fifteen to forty drops of an highly recommended aromatic syrup of galls, prepared according to the following formula :—Half an ounce of powdered galls, two drachms of powdered cinnamon, and half a drachm of powdered ginger, mixed in half a pint of the best brandy. Let the ingredients stand in a warm place for two hours, and then burn off the brandy, holding some lumps of sugar in the flames. Strain through blotting-paper. Tonics and stimulants, of which quinine and old brandy are the most reliable, are often necessary at an early period of the attack. "Small quantities of animal broth are proper, or the child may be allowed to suck pieces of juicy meat, or to eat very finely mixed meat of chicken or mutton," after the acute symptoms have ceased. In the treatment of the vomiting, so frequently a troublesome symptom, lime-water and milk with

a little brandy, or the following is prescribed:—Solution of sulphate of morphia, thirty-two minims; dilute sulphuric acid, fifteen minims; curacoa, two drachms; water, fourteen drachms. Dose, a teaspoonful every hour or two hours, at the age of six months to a year.

In the treatment of chronic entero-colitis and of dysentery, Drs. Meigs and Pepper mention aromatic sulphuric acid; opiate suppositoris; injections of nitrate of silver and liq. ferri. pernitr., as well as their internal use. They mix from ten to twelve drops of liq. ferri. pernitr., in four ounces of tepid water for each injection; and of nitrate of silver, gr. i. (gradually increased), to the same quantity of distilled water. “After the enema has come away it is a good plan to throw into the bowel a starch and laudanum injection.” Raw-meat and the yolk of a hard-boiled egg, have also been given with good results. The following extract which concludes Meigs and Pepper’s chapter on this disease, will show the lucid style of the authors, and the complete and judicious manner in which their modes of treatment are indicated:—

“It should never be forgotten in the treatment of chronic diarrhœa in children, that the most important point of all is the regulation of the diet and other hygienic conditions. We are fully convinced that we have seen several children saved from death by attention to these points, and by the persevering and careful employment of tonics and stimulants. It often happens, after the disease has lasted for some weeks or months, that the powers of the stomach are almost wholly lost. The child either refuses food or takes so little that the quantity is evidently insufficient to carry on the vital processes, or the greater part of what is taken is rejected by vomiting, or lastly, much of it passes off through the bowels, and appears in the stools in an undigested state, forming what is called lientery. If this condition of things is allowed to continue, the emaciation and exhaustion make rapid progress, and the case must soon terminate fatally. Under these circumstances all the ingenuity and skill of the physician are required to find articles of diet of a kind to recall and tempt the child’s worn-out and often perverted appetite, and which, at the same time, may be digestible and nutritious, and tend to restore vigor to the digestive function. If the stomach is frequently sick, it is best to abandon all remedies but those which are stimulating and strengthening, and especially to forbid all such as are in the smallest degree nauseous. We would indeed depend entirely on the use of repeated doses of the oldest and most delicate brandy that could be found, of which from one to two teaspoonfuls may be put into a wineglassful of cold water, and

the whole given by teaspoonfuls in the twenty-four hours; or fifteen to twenty drop doses of the elixir of Peruvian bark every three or four hours may be used; or Hawley's wine of pepsine, in half teaspoonful doses three times a day; or, two or three drops of tincture of nux vomica in sweetened water three times a day, if the bitterness does not cause nausea or increase the loathing. In such cases, wine of iron, in doses of twenty drops to a fourth of a drachm, with syrup of tolu and caraway water, will sometimes do exceedingly well; or the following, which has sometimes succeeded in our hands:

R.—Tr. Ferri. Chlorid.,	-	-	f3j.
Acid Acet. Dil.,	-	-	f3j.
Liq. Ammon. Acetat.,	-	-	f3ij.
Syrup Simp.,	-	-	f3ss.
Aquæ,	-	-	f3ij.—M.

"Dose at four years, a teaspoonful, and under that age, half a teaspoonful three or four times a day.

"In some very obstinate cases, especially where there is any reason to suspect the existence of a malarial element in the case, from half a minim to one minim of Fowler's solution of arsenic, with the wine of iron, three times a day, has been very serviceable. While this is being done, an occasional dose of anodyne, just enough to tranquilize without stupefying may be given. If the rectum will retain it, it is better to give it by enema. In some cases we have found the aromatic syrup of galls given with brandy, to be taken by the child without any difficulty or disgust.

"Exercise by riding and exposure to the air, which, however, must never be carried so far as to induce positive fatigue, are all-important. In some very severe and tedious cases, change of residence or travelling has been known to effect a cure after all remedies and other means had failed."

Hooping-cough, Vogel says, "is an epidemic contagious bronchial catarrh, with peculiar convulsive paroxysms of cough." According to Drs. Meigs and Pepper it "consists in slight inflammation of the respiratory mucous membrane, and a disordered action of the respiratory system of excito-motor nerves. It is neither a pure neurosis nor a pure inflammation, but partakes of the characters of both, and much more of the former than of the latter." In its treatment they consider the effects of alum, as recommended by Dr. G. Bird, more decided and satisfactory than those of any other remedy, especially when combined with belladonna; to a child one

year old they give, every sixth hour, a teaspoonful of a mixture consisting of extract of belladonna, one grain, alum thirty grains, syrup of ginger, syrup of acaciæ and water of each an ounce.

Carbonate of potassa is also a medicine of great value, and will succeed, in some instances, where the alum fails. The benefit of inhalation of the fumes of gas-works is corroborated; and the sudden application of a piece of ice, wrapped in linen, to the epigastrium, as also Dr. Churchill's plan of giving a whiff of ether or chloroform, in the paroxysms recommended.

Vogel notices Jenner's interesting observation on the prophylactic and abortive influence of vaccination on whooping-cough; an idea which is carried out to such an extent by Lachmann, that in already vaccinated children he administers a vaccine crust, rubbed up with sugar and milk, *internally*, and repeats the dose after four days; by which treatment rapid cures are claimed to have been achieved.

The importance, in consequence of their frequency, of bronchitis and pneumonia in children during the first five years of life, whether as primary or, more commonly, secondary affections, as in the course of measles, whooping-cough, enteric or scarlet-fever, &c., was first forcibly pointed out by Dr. West. At no period does bronchitis occur oftener than at the time of first dentition, and Vogel states that this is mainly due to the large quantity of saliva then secreted, dribbling from the mouth and soaking through the clothes covering the chest, and thereby producing a diminution of the temperature of the breast; as a result of which, swelling and an increased secretion of the bronchial mucous membrane ensue. This can easily be prevented by employing large sized slaving bibs lined with thin gutta-percha cloth or oil-silk.

The subjects of collapse of the lung, bronchitis, and pneumonia, are intimately connected, and until lately there has been much confusion between them. Vogel, on these points, gives us but scanty information, and indeed does not allude to atelectasis, except as affecting the newly born child, and in connexion with the rachitic thorax; omitting any mention of the researches of Legendre or Gairdner. He divides pneumonia into lobular and lobar; "the former," he says, "an extremely frequent affection." His description of it, however, appears to be identical with that of collapse of the lung, with its usual combination of bronchitic inflammation and pulmonary congestion; and this idea is strengthened by his stating in his chapter on whooping-cough, that lobular pneumonia is the

most frequent complication of the disease, collapse being, in fact, the most usual. Collapse of the lung occurs almost always, according to Drs. Meigs and Pepper, in connexion with bronchitis, although sometimes also, after, or concomitantly with pneumonia. It is divided into a lobar and lobular form; its physical signs—which in the lobular form, are very slight or entirely absent—are: “the existence of dulness, greater or less, on percussion; feeble respiratory murmur; prolonged expiratory sound, and sometimes bronchial respiration.” It ought to be suspected:

“Whenever, in a chronic disease, and especially in the course of a catarrhal attack occurring in a feeble and debilitated child, the breathing becomes excessively quick and laboured, the skin pale and coolish, when the base of the thorax presents a depression instead of an expansion during inspiration, and, especially, when these symptoms occur without there being a sufficiently severe and extensive bronchitis to explain their existence.”

And is usually distinguishable from pneumonia and pleurisy—

“By the absence in collapse, or the slight severity, of the reactional symptoms, by the paleness or blueness and coolness of the surface, by the absence of acute pain, by the greater severity in collapse of the bronchitic symptoms, and by the fact that it rarely occurs except in enfeebled, broken-down subjects, or in those labouring under severe bronchitis. The character of the physical signs, moreover, is different. Though we have dulness on percussion in collapse, it is not so absolute as that either of pleurisy with a large effusion, or that of confirmed pneumonia. The bronchial respiration, too, is in collapse, different from that of pneumonia. It is muffled and distant, instead of being clear, metallic, and close under the ear, as in pneumonia: and is heard, too, much more in the expiration than in inspiration. In collapse there is also heard, unlike either pneumonia or pleurisy, an abundant subcrepitant râle. To add to these differences, it is proper to say that, in cases of pneumonia and pleurisy, the course of the disease is much more regular, and the special symptoms so well marked as to leave no doubt as to the real nature of the attack.”

The not uncommon occurrence of abscesses of the lungs in the pneumonia of children, occasionally producing pneumothorax, is noticed by Meigs and Pepper, and some interesting cases simulating cerebral and meningeal disease given; showing the necessity in obscure cases for a most careful physical examination; neither authors,

however, make any mention of the information to be gained by the use of the thermometer in these doubtful and perplexing cases, which is much to be regretted, recent writers having proved the advantage and practicability of applying this means of diagnosis to the diseases of children.*

As the most striking symptom of pneumonia, and, along with increased action of the facial muscles (expansion of the *alæ nasi*, &c.), more pregnant with information than the results derived from the physical examinations which are usually fruitless, Vogel notices great acceleration of respiration (60 to 80 per minute) with an inverted rhythm, viz., the accent falling upon the expiration, which is louder than inspiration. This is also called expiratory respiration. In the treatment, Drs. Meigs and Pepper are of opinion that depletion should not be used save in very exceptional cases, with such marked indications as those laid down by Niemeyer. (*Text-Book of Practical Medicine*, Revised edition, Vol. i., p. 186). Dr. Vogel objects to it and the use of antimony *in toto*, but gives calomel, one-eighth of a grain four times daily, "when the skin is burning hot, and no diarrhœa is present, until green semi-fluid stools ensue; and as an emetic, a few teaspoonfuls of an infusion of ipecacuanha (ʒi. to ʒi. water), only if the dyspnœa is urgent. In all his cases the local treatment consists in the application of tepid compresses, thus: a large white pocket-handkerchief is folded up like a cravat, dipped in tepid water, wrung out and applied like a girdle round the chest; this is covered by a piece of gutta-percha, and another large double cloth placed dry and warm over all. Drs. Meigs and Pepper never give calomel or tartar emetic. Citrate of potash (gr. ss. to gr. ijss. every two hours) either alone or with small doses of syr. ipecac. and opium is, they consider, an excellent remedy; and they strongly recommend mustard poultices and foot-baths. Opium is constantly of service, and tonics and stimulants to be resorted to in cases "which manifest undoubted signs of debility."

The greatest confusion has arisen—and, indeed, is still too prevalent—from not making a careful distinction between croup or pseudo-membranous laryngitis, and spasmodic simple laryngitis or pseudo-croup. In some grave forms, however, of the milder disease it is impossible to make the diagnosis with certainty, although in general it is a simple affection and readily cured by warm-baths, an alkaline mixture, and emetic of ipecacuanha; but it should never be

* Bouchut, *Traite Pratique des Maladies des Nouveau Nes*. W. Squire, *Temperature Variations in Diseases of Children*.

regarded slightly as it may pass into genuine croup. Pseudo-croup is very much disposed to relapse, a rare occurrence in true croup, and is, further, mainly distinguished from pseudo-membranous laryngitis, by the fever not being continuous, seldom lasting over three days, being usually amenable to treatment, and rarely fatal. The paroxysms generally recur at night, and are often averted by a moderately full dose of an opiate, such as Dover's powder at bed-time.

Concerning the much disputed question as to the relation, if any, between croup and diphtheria, Meigs and Pepper seem to have come to a clear and, we think, satisfactory conclusion. They define true croup to be "an acute inflammation of the larynx attended with exudation of false membrane." Diphtheria, as "an acute febrile, moderately contagious, and infectious asthenic blood disorder, occurring also endemically and epidemically, without characteristic eruption, and distinguished by a disposition to the formation of false membranes upon inflamed mucous surfaces, especially in the fauces, or upon abrasions of the cutaneous surface." Although not denying the possibility of pseudo-membranous laryngitis occurring as a purely primary idiopathic affection, they are, nevertheless, of "decided opinion that the vast majority, if not all, of the cases usually termed pseudo-membranous laryngitis are, in reality, instances of primary laryngeal diphtheria, in which the constitutional symptoms are not grave, and where the faucial deposit has been very slight and, perhaps, overlooked." Again—

"It is a well-recognized fact that true diphtheritic croup is nearly always preceded or accompanied by pseudo-membranous exudation in the fauces or pharynx, but the amount of deposit in these latter places may be extremely small and yet be followed by extensive exudations in the air-passages; while, on the other hand, there is often copious deposit upon the pharynx in cases where the larynx does not become invaded."

Both authors agree that there is no essential difference in the anatomical conditions between primary and diphtheritic croup. Vogel states that the symptoms of genuine croup "may depend upon *three different* kinds of processes upon the mucous membrane of the larynx. The exudation poured out by the inflamed mucous membrane, may be either—(a) muco-purulent, or (b) simply fibrinous, or (c) diphtheritic," and that the reason for the diverse statements concerning croupous angina and diphtheritic croup "is found in the diversity of the anatomo-pathological process." Croup,

therefore, he concludes, "is no local laryngeal affection, but a general disease, a toxæmia, perhaps, with localization upon the larynx," and the prognosis in well declared cases, fatal. The treatment of croup is never likely to succeed unless commenced early. Drs. Meigs and Pepper regard emetics as the most important remedies; they, however, agree with Vogel in discountenancing tartar emetic, and strongly recommend alum given in powder in doses of a teaspoonful mixed in honey or syrup, to be repeated (which is seldom necessary) until it operates. Alum "is certain and rapid in its action, without producing exhaustion or prostration." They give the above dose every four or five hours for two or three days, and have never observed any bad effect from it. Opium is the best remedy for the laryngeal spasm, and ice in small pieces, given freely, acts very favourably. As to the local treatment of diphtheritic croup, Vogel has abandoned cauterizations altogether, but has derived remarkably good results from "the inhalation of pure lime-water, five to six times daily for at least five minutes at a time, by the now very simplified *pulverisateur*" in children who may be induced to submit to it. Meigs and Pepper also recommend lime-water, but not to the exclusion of solution of nitrate of silver (gr. x-xx to the ounce), or hydrochloric acid.

With regard to tracheotomy, Vogel discourages the operation on the ground of croup being a general disease. Meigs and Pepper come to the conclusion that—

"The operation is justifiable despite any complications which may co-exist, save perhaps the presence of grave general diphtheritic infection; and finally that, when no such contra-indication is present, and the dyspnœa is continuous and increasing despite all other treatment, the operation is positively indicated, and it becomes the duty of the practitioner to recommend its performance, and, if the decision be entrusted to him, to unhesitatingly assume the responsibility of operating."

In connexion with this subject, the recently published experiences of Mr. Spence,^a who has operated on 91 cases of simple and diphtheritic croup, with an average recovery of one out of three cases; and of Dr. G. Buchanan,^b who has had the same high ratio of success in 39 operations, may be cited.

In a reprint from the *American Journal of Obstetrics and Diseases of Women and Children*, for May, 1870, Dr. Barker gives his plan of treatment in croup—excluding the croup of diphtheria, which he regards "as an entirely distinct and specific affection"—with which

^a Lectures on Surgery.

^b British Medical Journal, March 25th, 1871.

he claims to have had unusual success, "never having lost a case during twenty years' practice." He commences with an emetic of turbeth mineral (*hydrargyri sulphas flava*) in doses of from three to five grains according to age, and strongly insists upon the importance of keeping this medicine at hand, in the houses of all families with young children who have manifested "the slightest tendency to catarrhal laryngitis," so as to lose no time in the administration of the remedy "immediately on the appearance of the symptoms which threaten croup. If the supposed attack of croup is simply one of what is called false or spasmodic croup, the powders do no harm;" but if acute febrile symptoms continue, he prescribes *tinct. verat. viridi.* in one or two drop doses until the pulse falls to 80, and, if necessary, repeats the emetic. To account for such wonderful results, we may remark that Dr. Barker cannot recognize any practical difference between false and true croup, regarding them as an identical disease, "only differing as to intensity and extent of tissue involved." We quite agree with Dr. Barker as to the importance of early treatment, and have no doubt that some of his cases were ones of genuine fibrinous croup, as he mentions that "several times a few hours after the emetic, but never during its immediate action, the child has thrown off more or less detached portions of membrane;" but would much wish for further evidence as to the success of this mode of treatment. He alludes to the performance of tracheotomy in an advanced stage of the disease, but does not say whether he ever had to resort to it in any of his cases.

Typhus abdominalis (enteric fever) and *cholera Asiatica*, are treated of by Dr. Vogel under the head of "diseases of the digestive apparatus," as being "infectious diseases with predominating localization upon the intestinal canal." By Meigs and Pepper, typhoid fever is described under "eruptive fevers," and *cholera infantum*, under "diseases of the digestive organs." This classification of both authors is open to objection, but Vogel's chapter on abdominal typhus is, we consider, one of the most complete and exhaustive in his book. That a large number of cases which are, in reality, due to enteric fever, are often—owing to its imperfect recognition as a frequent infantile disease, and from the remissions in temperature during its course—attributed to, and confounded with, other affections, such as infantile remittent, or the so-called "gastric fever," is still undoubtedly the case. The diagnosis also between enteric fever, and cases of gastro-enteritis,

and the early stage of acute general tuberculosis (pulmonary, meningeal, or intestinal), is so often attended with the greatest difficulty, that Vogel says the treatment for the first few days, until the diagnosis is clearly made out, must be purely expectant.

The liability of children to the infection of enteric fever, if such can be assumed, is, according to Vogel, extremely small. Meigs and Pepper also say that "the degree of its contagiousness is extremely slight." The reason why intestinal hæmorrhage and perforations are so extraordinarily rare in children, given by Vogel, is "that a majority of Peyer's patches always stop at the stage of the brain-like infiltration." He also gives the following simple account of the origin of the miliary vesicles so commonly seen:—

"As a result of the cessation of the perspiration at the commencement of the fever, the epithelial cells lining the excretory ducts of the sudoriparous glands become dry, are cast off, but not washed away, forming a dam, which the perspiration, that has been arrested for some time, but which now, suddenly re-established and profusely secreted, is unable to break through, thus causing an elevation of the occluded orifice, and the similarly desiccated layer of epidermis surrounding it, to the extent of a pin's head in circumference. After two, or, at the longest, three days, these epidermal caps burst, and the perspiration oozes out uninterruptedly through the once more pervious and cleansed passage."

The therapeutics of the disease laid down by Vogel are particularly clear, and, we think, most judicious; in the treatment of the severe forms, he says that—

"The best remedy for the fever and congestion of the head is cold. The means by which this is accomplished are: cool temperature of the room, from 58° to 64° F., light coverings, hair cut short, pillow of horse-hair encased in soft deer-skin, hourly cold douching of the head, cold affusion of the whole body, once or at most twice daily, and a bladder filled with ice to the head—this, however, is applicable only to large children when not delirious. . . . The best remedy for the excitement, sleeplessness, and delirium, in this disease, is laudanum. One drop less than the number of years of the age of the child should be given; thus, to a child three years of age, two drops, to one four years, three drops, &c.; and this may be repeated twice or thrice daily.

"Against the great exhaustion, feeble pulse, cool, bluish skin, indicating supervening splenization of the posterior parts of the lungs, a tonic and stimulating treatment will have to be employed, in which I give to coffee the most prominent position; wine, which, in adults, justly plays

such an important role, must be used very carefully. . . . Dry cups, applied several times a day to the anterior and lateral parts of the thorax, are not only theoretically rational, but practically exercise a tolerably favourable influence upon the splenization."

In concluding this protracted notice, in which, nevertheless, we have been obliged to omit, from want of space, mention of many other interesting and instructive subjects, especially "nervous diseases," and the "cachexiæ;" we would wish to commend particularly the chapters by both authors on the "examination of children," as being replete with most useful practical information, without an acquaintance with the details of which, on the part of the practitioner, inexperienced in the Pædiatria, "a successful treatment would be clearly impossible, notwithstanding all his knowledge and skill in the methods of examination."—(Vogel).

On Some Disorders of the Nervous System in Childhood: being the Lumleian Lectures delivered at the Royal College of Physicians of London, in March, 1871. By CHARLES WEST, M.D., Fellow and Senior Censor of the College; Physician to the Hospital for Sick Children. London: Longmans, Green, and Co. 1871.

IN this little volume we have from the pen of Dr. West an account of some of the nervous diseases which are incident to childhood. The work consists of three lectures, delivered at the London College of Physicians. The first lecture treats of neuralgia and epilepsy, the second of chorea and paralysis, and the third of disorder and loss of power of speech, mental and moral peculiarities, and their disorders.

These lectures, without containing much that is novel, and without being either very copious or exhaustive in their discussion of the subjects to which they are devoted, are eminently worthy of perusal, from the wise and thoughtful spirit in which they have been conceived, and from the admirable practical sense and sagacity which characterize all the writings of their distinguished author.

On the subject of neuralgia we have some very judicious and suggestive observations. Dr. West, after alluding to the frequency with which functional nervous affections are met with in the adult, and their long continuance without the supervention of any organic disease, observes:—

“In infancy and childhood, however, pain referred to any part signifies almost without exception that disease of some sort or other is going on there, or near at hand. The tears so profusely shed do not prove that pain is the lot of the infant more than of the grown person: but at one time cries are the only, as they long continue the most expressive, language. Hunger, sleeplessness, fatigue, discomfort of any sort is expressed by cries; while the character of the cry goes far towards helping us to determine the nature of the suffering. But I have never in infancy known any instance of pain—severe, obstinate, recurrent—for which, sooner or later, a distinct local cause was not found; and even in later childhood the rarity of real neuralgia is extreme.

“There are two classes of cases in which it is of especial importance to bear in mind this caution: the one those cases in which pain is referred to the head; the other those in which it is situate in one of the lower limbs. In the former case the pain is almost invariably symptomatic of organic disease of the brain: in the latter with almost equal certainty of hip-joint disease. And yet I have often seen it regarded for many days as purely neuralgic. The mistake is the more likely to be committed when pain is referred to the head, owing to the undoubted rarity of intense pain as a symptom of acute disease of the brain, while the severe suffering which sometimes attends cerebral tubercle is almost always associated with some positive symptom or other of organic mischief. But now and then I have seen cases where, after some previous failure of health, and some trivial head discomfort such as scarcely attracted attention, pain has come on, so sudden, so violent as to throw all other symptoms completely into the background, so distinctly and almost completely intermittent, and for a time, even under the influence of quinine in large doses, as to mislead even the most wary. And yet time has undeceived one as to the nature of the case; the intermissions have become less complete, and of shorter duration; the influence of quinine has passed away; the intervals between the paroxysms of pain have no longer been times of cheerfulness, but of indifference to objects around, till the indifference has deepened into stupor, and suffering has abated just in proportion as consciousness has been lessened.

“It is most difficult to lay down rules for the avoidance of error, for while it is undoubtedly true that neuralgia may follow either on some previous ill-defined feverish attack, or may take place during convalescence from typhoid fever, it is just in such conditions that real disease of the brain oftenest comes on; and the latter is of far more frequent occurrence than the former. It may, however, be of some use to bear in mind that neuralgic pain is localized in some part of the head; that, while it is very intense and accompanied with excessive intolerance of light and sound, it is also often attended with weeping, and the importance of tears as disproving the existence of real inflammatory disease

either in the head or chest, first dwelt on by Trousseau, cannot be over-rated. The intervals between the paroxysms are times not only of perfect ease, but of cheerfulness; sickness is absent, the power of taking food is not lost, and sleep, if not interrupted by pain, is quiet and refreshing. Moreover, there is no dizziness, though there may be heat of head; the pulse is unusually quick and feeble, and, I must add, may be irregular or actually intermittent, for while, as a general rule, irregularity of the pulse is one of the least invariable symptoms of disease of the brain, there are some children with whom any disorder of the nervous system, especially such as is sympathetic with disturbance of the digestive organs, is invariably attended with irregularity of the heart's action.

"Pain, dependent on real cerebral disease is rarely limited to one part of the head; or, if it be, is referred to the forehead. It is generally, though not invariably, less intense, the intermissions of suffering are less complete, and some one symptom almost always attends the pain: it may be sickness or obstinate constipation, or dislike of light or sound, even when the pain abates—some one symptom, small in itself, but enough to keep alive the anxiety of anyone who subscribes to Morgagni's saying, that 'the habit of observation is the foundation of the art of Medicine.'"

Dr. West is of opinion that there are no adequate grounds for the opinion, that there is any invariable organic change in chorea. With reference to the connexion which undoubtedly exists between rheumatism and chorea, he draws attention to the fact that before puberty the female is found to be more liable to rheumatism than the male, although in the adult the male is the more liable. Of 520 children under the age of 12, and mostly under the age of 10, admitted into the Children's Hospital suffering from rheumatism, only 243 were males, while 277 were females.

It is interesting to observe that the author has observed only one instance in which chorea could be attributed to mental work, and he is inclined to believe that fright is only rarely the cause of the attack. In the treatment of chorea Dr. West has observed decided advantage to result from the employment of tartar emetic in the manner recommended by Dr. Gillette, of Paris, but the only remedy which he thinks has shown specific power over the disease is sulphate of zinc, administered in increasing doses. He begins with small doses and increases them rapidly, tolerance being speedily established. He has found 10, 15, or 20 grains, taken four times a day, without any unpleasant symptoms.

We cannot better conclude this brief notice than by quoting the tender and eloquent remarks on the mental and moral peculiarities of childhood:—

“Now the child differs essentially from the adult in these respects ; that

“1st. He lives in the present, not in the future.

“2nd. His perceptions are more vivid, and his sensibilities more acute, while the world on which he has just entered surrounds him with daily novelties.

“3rd. He has less self-consciousness, less self-dependence, lives as a part of the world by which he is surrounded—a real practical pantheist.

“The child lives in the present, not in the future, nor much even in the past, till the world has been sometime with him, and he by degrees shares the common heritage of retrospect and anticipation. This is the great secret of the quiet happiness which strikes almost all visitors to a children’s hospital.

“No one can have watched the sick bed of the child without remarking the almost unvarying patience with which its illness is borne, and the extremity of peril from which, apparently in consequence of that patience, a complete recovery takes place. Much, indeed, is no doubt due to the activity of the reparative powers in early life, but much also to the unruffled quiet of the mind. No sorrow for the past, no gloomy foreboding of the future, no remorse, disappointment, nor anxiety depresses the spirits and enfeebles the vital powers. The prospect of death, even when its approach is realized—and this is not so rare as some may imagine—brings in general but small alarm. This may be from the vagueness of the child’s ideas ; it may be, as the poet says, that in his short life’s journey ‘the heaven that lies about us in our infancy’ has been so much with him that he recognizes more clearly than we can do

“‘the glories he hath known,
And that imperial palace whence he came.’”

PART III.

MONTHLY REPORTS

REPORT ON OBSTETRIC MEDICINE AND SURGERY.^a

By GEORGE H. KIDD, M.D., F.R.C.S.I., L.K. & Q.C.P.;
President of the Dublin Obstetrical Society; Honorary Fellow
of the London Obstetrical Society; Corresponding Member
of the Gynæcological Society of Boston; Obstetric Surgeon to
the Coombe Lying-in Hospital.

DISEASES OF THE OVARIES.

NOTWITHSTANDING the great progress that has been made in our knowledge of ovarian disease, there are still many difficulties to be contended with, both as to diagnosis and treatment. Tumours have been cut down upon as ovarian which proved to be in no way connected with these organs, and true ovarian tumours have been operated on which were found to be so malignant in their nature that their removal afforded no benefit to the patient.

FIBRO-CYSTIC TUMOUR OF THE UTERUS.

In the early history of ovariectomy many cases are to be found in which, when the abdomen was opened, the tumour was found to be not ovarian at all. Errors of this sort are happily every day becoming more rare; but there is one form of tumour, the fibro-cystic of the uterus, the true diagnosis of which has not yet been established, and which may still be mistaken for an ovarian tumour. Dr. Charles C. Lee^b has collected nineteen cases of this form of disease from the records of the practice of men of the highest renown, which were subjected, after the most careful examination, to operation, and in only one of which the true nature of the case

^a The author of this Report, anxious that every contribution to Obstetric Medicine and Surgery should be noticed, will be glad to receive publications on the subject. They may be sent to the publishers of the Journal through their correspondents.

^b Remarks upon the Diagnosis of Ovarian Tumours from Fibro-cystic Tumours of the Uterus.—New York Journal, Nov., 1871, p. 449.

was known beforehand. In fifteen the operator did not entertain a doubt of the correctness of his opinion, that the disease was ovarian, till the operation had reached a point from which retreat was impossible, and in three it was suspected that the growth might possibly be uterine, but so faint was the suspicion that the operation was undertaken as if the disease were really ovarian.

Dr. Lee gives the following account of this fibro-cystic tumour of the uterus, or uterine tumour that has undergone cystic degeneration, and which is, he says, an exceedingly rare growth, and one that is essentially distinct from the ordinary fibroid in whose substance minute fluid collections have formed:—

“The anatomical structure of these formations is minutely described by Dr. C. G. Ritchie, from dissections of specimens removed by Spencer Wells. Their pathology was first studied by Cruveilhier, who delineates their appearance, and divides them into two classes. One of these consists of irregular cysts with undeveloped walls, due apparently to an œdematous infiltration of the fibroid, the ‘interlobular spaces’ of which gradually dilate and finally give way from the pressure of the interstitial liquid. Anfractuous cavities are thus formed, more or less closely resembling true cysts, filled with a limpid serous liquid, generally of a light-yellow hue, but sometimes bloody. In the second variety are found perfect cysts of smooth walls, which Kœberlé considers due to the progressive dilatation of the lymphatic vessels. The cavities usually communicate with each other, the larger ones being spherical in shape, while the smaller accessory cavities are flattened or shaped like irregular sinuses. Their liquid contents are yellowish, limpid, fibrinous, and spontaneously coagulable.

“Mr. Paget thinks the cysts are due either to a ‘local softening and liquefaction of part of the tumour, with an effusion of fluid in the affected part; or to an accumulation of fluid in the interspaces of the intersecting bands; and these are the probable modes of formation of the roughly-bounded cavities that may be found in uterine tumours.’”

The case in which the true nature of the disease was recognized occurred in the practice of Prof. Kœberlé, and of it Dr. Lee gives the following account, and closes his communication with a tabular statement of the chief means of diagnosis as drawn from the cases he has recorded:—

“Prof. Kœberlé, of Strasbourg, has, however, published in the *Gazette Hebdomadaire* a paper characterized by great ingenuity and research, in which he claims that the diagnosis of fibro-cystic growths can with certainty be established by attention to the following points:

"1. The discoloured hue and dejected expression of the face, or the so-called *facies uterina*, of the patient.

"2. The variable consistence of the growth, as shown by abdominal palpation.

"3. The results of tapping. If the trocar touch a fibrous spot in the tumour-wall, blood will flow; even when the cyst is reached, the fluid never presents the clear, viscid character of ovarian cystic fluid, but is either yellowish, thin, serous, and rich in lymph or cholesterin, or it is brownish, muddy, sero-purulent, or bloody.

"4. The indurated (or nodulated) feeling of the tumour after tapping.

"5. The uterine connexions of the growth, as made out by careful vaginal and uterine examination.

"By these means M. Kœberlé was able to diagnosticate with precision the following interesting case, which is reported in the same connexion:

"CASE XIX.—An unmarried lady of Wiesbaden, aged 34, consulted him in 1868 for an abdominal tumour, which was first observed two years previously; although three years before that time her health had become impaired by excessive constipation and other irregularities which seemed due to pelvic obstruction.

"During the last year the abdomen had rapidly enlarged, and, at the date of consultation, was quite filled by a rounded tumour, fluctuating at some points, solid at others, and giving the general impression of a multilocular ovarian cyst. The pelvic cavity was also filled by the mass which seemed continuous with the posterior part of the *cervix uteri*, which was pushed forward, and to the left. The hymen was so tight and dense as to preclude the use of the sound.

"Two of the largest compartments of the tumour were tapped, and yielded two and a half quarts of a serous fluid, containing large quantities of cholesterin. This fact, with the irregular outline of the tumour, its close connexion with the *cervix uteri*, and the marked *facies uterina* of the patient, convinced him that it was a fibro-cyst of the uterus, in spite of its rapid development and multilocular character, which indicated an ovarian origin.

"The patient, who had undergone a variety of useless treatment, imperatively demanded an operation, from which M. Kœberlé tried in vain to dissuade her; but, after fully explaining its risks and the improbability of a successful termination, he consented to attempt gastrotomy. Operation, August 31, 1868; anæsthesia by chloroform. Short median incision, afterwards enlarged. When exposed, the tumour was lifted with great difficulty from the abdominal cavity; and, after numerous tapplings, which obtained only three and a half quarts of fluid, it was found to spring directly from the posterior uterine wall, without involving the neck or fundus. Both ovaries were healthy.

"The pelvic portion of the tumour, which was comparatively very

small, formed a kind of pedicle for the rest. The punctures made by the trocar bled so freely that an iron-wire ligature was thrown around the pedicle as close to the womb as possible, and the abdominal tumour excised. The pelvic portion was separated with extreme difficulty, being intimately adherent to the recto-vaginal *cul-de-sac*, and the posterior vaginal wall; it was gradually enucleated, without injury to the pelvic vessels, and the womb left intact. Hæmorrhage very copious; arrested by metallic ligatures cut short, the actual cautery, and perchloride of iron.

"The operation lasted two and a half hours, and the patient was exceedingly cold and feeble; reaction was gradual but complete, and in twenty-nine days she was walking about, and soon returned home.

"A careful histological examination of the tumour showed conclusively its uterine origin and its fibro-cystic structure.

"It is not to be supposed that every case of fibro-cyst of the uterus will exhibit all the distinctive marks noted by Kœberlé, and a glance at the cases already detailed will show the need of bearing other points equally in mind.

"For instance, as to time or progress of development—although generally slower than ovarian growths, this will depend upon whether the original fibroid is interstitial or merely subperitoneal. In the latter case, its cystic degeneration and growth may be quite as rapid as ovarian disease.

"Again, menorrhagia, as a forerunner or coincident symptom, is seldom found to exist in fibro-cystic disease, because the neoplasm is extra-uterine from the beginning, at least only slightly invading the uterine wall, whereas the true fibroid is more deeply seated. The disregard of this fact, and the fictitious value attached to the absence of flooding, have powerfully contributed to errors in diagnosis.

"Thirdly, independent mobility of the womb really indicates nothing but the absence of pelvic adhesions; for, if the fibro-cyst have passed into its second stage of development, and undergone extensive cystic degeneration, it will scarcely ever be affected by uterine motion—especially if a moderately small pedicle exist; and lastly, the uterine hypertrophy or increased length of the cavity, upon which much reliance has been placed, is shown to be of very doubtful value by Dr. Routh, who has found the greatest amount of elongation in certain ovarian cases.

"M. Kœberlé is of opinion that fibro-cystic disease never appears under thirty years of age, although among the preceding cases, two exceptions to this rule are found. With these general facts in mind, the differential diagnosis between uterine fibro-cysts and ovarian cystic tumours may be pretty confidently stated as follows :—

IN OVARIAN CYSTS.

1. Disease may occur at any period, even before puberty.
2. Development rapid—usually under two years.
3. Aspect of face unaltered, if general health be fair.
4. Fluctuation equable over whole surface of tumour.
5. Vaginal examination shows little displacement of uterus—mass smooth and distinct from uterus.
6. Mobility of uterus independent of tumour from beginning—pelvic adhesions rare.
7. Tapping causes complete collapse of unilocular cysts—in polycystic tumours, it reveals the endocysts.
8. Fluid clear, straw-coloured, serous, or viscid, clear, mucoid, albuminous.
9. When exposed by gastrotomy, sac is pearly blue, or white and glistening; rarely vascular.

IN FIBRO-CYSTS OF THE UTERUS.

1. Scarcely ever occurs under thirty—generally from forty to fifty.
2. Development slow; generally over two years.
3. "Facies uterina" generally marked; expression anxious and dejected.
4. Fluctuation confined to certain regions—generally to upper portion, while lower is hard and dull.
5. Vaginal examination shows uterus high up or displaced. Mass either not detected, or continuous with uterus.
6. Independent mobility of womb confined to last stage of disease. Pelvic adhesions common.
7. Tapping causes only partial collapse, leaving base of tumour firm and indurated.
8. Fluid either brownish, bloody, seropurulent, muddy; or thin, yellowish, containing shreds of lymph or cholesterolin.
9. Exposed sac dark, vascular, thick, and frequently fasciculated with fibrous bands.

In a communication made to the British Medical Association at their last meeting, by Dr. Beatty,^a of this city, the particulars are recorded of a case of fibro-cystic tumour, in which he was consulted as to the propriety of an operation, and, recognizing the true nature of the disease, advised against any attempt to remove the tumour.

The woman was under the care of Dr. Morgan, from whose description the following abstract is taken:—She was aged 22, and three years married. About a year after marriage she discovered a small tumour in the left iliac region, which remained of the same size for a year, when she discovered a similar one commencing in the right iliac fossa. In about three months these growths seemed to have coalesced into one, and had enlarged considerably, giving the appearance of a woman in about the seventh month of pregnancy. She stated that the tumour had rapidly gained this size, and then remained stationary for about three months; menstruation had continued rather profusely, and at intervals of three weeks. The tumour was dull on percussion, of an oval outline, and uniform

^a Remarks on Fibro-cystic Disease of the Uterus.—*British Medical Journal*, Nov. 4, 1871, p. 517.

to the feel. A deep fluctuation could be perceived. Tapped with the *aspirateur* there were removed 12 or 14 oz. of a dark coloured fluid, nearly of the colour of beer, of rather viscid consistence, and highly albuminous. She was tapped a second time, and after neither operations suffered any inconvenience. "She now underwent a rather profuse menstrual period of eight days' duration," and it was after this that Dr. Beatty saw her, and examined her case in company with Dr. Atthill. She was afterwards tapped, this being the third time, and three pints of fluid of the same character discharged, after which peritonitis set in and the woman died. Dr. Beatty describes, as follows, the appearances after death, and the grounds of his diagnosis:—

"*Post-mortem Examination.*—The body generally was in good condition; the abdomen was much distended by gas. There was a considerable amount of purulent matter in the cavity of the abdomen and around the tumour. There were some adhesions between the omentum and the tumour, and in three places these were tolerably firm. The surface of the tumour was uniform, and from the upper part a curious offset had grown, which had apparently given way and caused the peritonitis. There was no evidence of inflammation in the vicinity of the punctures. On raising the tumour, the ovaries were found in their normal position, and free from disease. The tumour was found to have grown from the upper part of the fundus uteri. The large cyst in immediate connexion with the uterus was opened, and showed the remarkable figure of the interior.

"When I examined the woman, the tumour in the abdomen resembled in size and shape the gravid uterus between the seventh and eighth month. There was evident fluctuation, and it seemed as if the fluid was in one large chamber, but the walls of the cavity felt thicker under the fingers than is the case in a single cyst of ovarian dropsy. There was a doughy feel when pressed deeply, as if thick flesh formed the walls of the cavity, and the tumour could be traced deep down into the pelvis. On examination *per vaginam*, the os and cervix uteri were found in a natural condition, and the sound passed in gave evidence of the normal size of the organ; but it was fixed, and would not be moved in any direction. The length of time the tumour had taken to arrive at its present development was well considered. This is an element observed upon by Dr. Graily Hewitt in his work on the *Diseases of Women*. He considers it as a diagnostic mark between uterine and ovarian tumours, the growth being slower in the uterine than in the ovarian cases. I was fortunate in the case with which I had to deal, for it was not of great size like that one in which Mr. Spencer Wells had operated at my

suggestion (the girth of which was fifty-six inches); but the walls, instead of being spread out and thinned by distension, had still a considerable amount of thickness, which gave the peculiar doughy feel to the fingers, while the cavity was still so large as to give a very distinct fluctuation. This symptom, though available to me on this occasion, would not be of equal value in a case wherein the walls had become thin by distension, and therefore I do not offer it as a diagnostic mark in all cases of fibro-cystic disease of the uterus, but I think it would be useful in those that had not attained a great magnitude. The profuse and repeated occurrence of menorrhagia suggested the existence of fibroids, and the fixity of the uterus, though natural in size and shape, showed that it was attached intimately to something above it, and that being a soft, fleshy-feeling, doughy tumour with fluctuation, led me to the conclusion that the case was one of fibro-cystic disease of the uterus, an opinion which I then communicated to Dr. Morgan."

In a recent paper,^a Mr. Spencer Wells has discussed, at some length, the diagnosis of uterine and ovarian tumours. He says some of the largest abdominal tumours he has ever seen have been fibroid or fibro-cystic tumours of the uterus, and more than a hundred cases are, he says, on record, where the abdomen has been opened with the object of removing an ovarian tumour, but the operator discovered, after making the incision, that the tumour was not ovarian but uterine. And, further, some of the tumours actually removed and believed by the operator to be ovarian, have been proved, on careful examination, to be really fibroid outgrowths from the uterus, more or less pediculated. In one case of a pediculated fibro-cellular outgrowth from the fundus uteri, he only discovered what he had done by finding both ovaries healthy; and when this tumour was exhibited at one of the societies, he had difficulty in convincing some of the Fellows that it was not ovarian.

Mr. Wells shows that these tumours may resemble enlargements of the ovary in position, shape, form, consistence, mobility, and sensibility. That there is nothing certain in the history to afford decisive assistance. That uterine hæmorrhage, though more common in uterine cases, may be associated with ovarian disease. That though uterine tumours are more common in old persons, and ovarian in younger, both may occur at all ages and in all conditions of life, married, single, or widowed. They may both occur in the

^a A Fourth Series of One Hundred Cases of Ovariectomy, with Remarks on the Diagnosis of Uterine from Ovarian Tumours. By T. Spencer Wells, F.R.C.S. Eng. Medico-Chirurgical Transactions, Vol. liv.

fat, healthy, and well nourished, or in the emaciated; and there is a facial expression common to women suffering from both classes of tumours, associated commonly with a very florid expression when the tumour is uterine. In the majority of ovarian cases the complexion is pallid, but, in some cases, when the patient is fat or well nourished, the complexion may be florid. Mr. Wells, acknowledging the numerous exceptions, just stated, to all the rules, makes the following remarks on the diagnosis of these tumours:—

“ Inspection.

“ 1. Visible enlargement of the abdomen is more often *general* in cases of ovarian tumour, and *partial* in cases of uterine tumour, being confined to the lower part of the abdomen until a very large size has been attained.

“ 2. The depression of the umbilicus is diminished, or the umbilicus may become prominent, in large ovarian cysts. This is rarely seen in uterine tumours unless fluid is also present in the peritoneal cavity.^a

“ 3. Enlargement of the superficial veins of the abdominal wall, and oedema of the abdominal wall and of the lineæ albicantes, are more general in uterine than in ovarian tumours of moderate size, but are not uncommon when ovarian tumours have attained a very large size.

“ When the abdominal wall is thin, both uterine and ovarian tumours, if not very closely adherent to the abdominal wall, may be seen to move downwards as a recumbent patient inspires, and upwards during expiration, falling downwards or forwards as she sits or stands, and more or less to either side according to the inclination of her body. But nearly all uterine tumours, though visibly moving above, seem to be fixed below in the hypogastric region.

“ 5. When a recumbent patient attempts to sit up without aid from any other than the abdominal muscles, the recti are seen to bulge forward in front of a tense non-adherent ovarian tumour or with a flaccid adherent cyst. This is seldom well marked in uterine tumours, a solid mass fixed centrally below the umbilicus interfering with the free action of the recti.

“ Measurement.

“ 6. Increase in the circular measurement of the abdomen is usually greater on one side than the other in ovarian tumours. In uterine tumours the increase is more often symmetrical. In both classes, vertical measurement shows the distance between the pubes and the sternum to be increased. But very great proportionate increase of the space from the pubes to the umbilicus is more common in uterine than in ovarian tumours.

^a In a case of very large fibro-cystic tumour of the uterus recently in the Adelaide Hospital, in this city, under the care of Mr. Walsh, a portion of the tumour protruded at the umbilicus, along with a piece of omentum, so as to form a large irreducible umbilical hernia.—G. H. K.

"Palpation.

"7. Large masses of apparently solid matter, and smaller masses or nodules of very hard or bone-like substance, are sometimes observed in ovarian tumours. But it is excessively rare to find such solid portions *preponderating* in an ovarian tumour. As a rule, the fluid or cystic portion is the larger, the hard or solid portion the smaller, in ovarian tumours. In uterine tumours, on the contrary, the solid is the larger, the fluid the smaller, portion.

"8. The mobility of ovarian tumours is generally greater from below upwards than that of uterine tumours, unless the latter are distinctly pediculated. If one hand be pressed backwards between the tumour and the pubes, an ovarian tumour can generally be raised considerably, and the hand can sometimes be pressed backwards almost to the brim of the pelvis; while a tumour which involves the body and neck of the uterus cannot be raised at all, or only with difficulty, and the hand cannot be pressed down between the pubes and the tumour.

"9. When there is fluid free in the peritoneal cavity, and a hard tumour can be felt on displacing this fluid by sudden pressure, the tumour may be either uterine or ovarian. If the tumour be very hard and the quantity of fluid small, the tumour is probably uterine and the fluid ascitic. An ovarian tumour which has given way, and emptied one or more of its cysts into the peritoneal cavity, is seldom hard or well defined in outline, and the quantity of fluid is often so large that the size and shape of the tumour cannot be ascertained until after removal of the fluid by tapping. The characters of the fluid will then complete the diagnosis.

"Percussion.

"10. As percussion elicits a dull sound all over both uterine and ovarian tumours, which dulness ceases abruptly at the border or outline of the tumour in all positions of the patient—except in the rare cases where a cyst contains gas, or where a coil of intestine is adherent in front of a tumour—percussion cannot afford much aid in distinguishing ovarian from uterine tumours.

"Auscultation.

"11. In ovarian tumours the impulse from the aorta is often perceptible, and a sound sometimes accompanies the impulse. The sounds of the heart are rarely transmitted, and any distinct vascular murmur is excessively rare. But in about half the cases of uterine tumours which I have examined some variety of vascular murmur may be heard. In some cases the murmur is tubular, in others vesicular, and sometimes a tubular and a vesicular murmur may be heard in different parts of a uterine tumour. These murmurs are synchronous with the pulse. They

may vary in intensity with the amount of pressure by the stethoscope, and may disappear on very firm pressure. Common in uterine, very rare in ovarian tumours, vascular murmurs are valuable aids in diagnosis.

“Having thoroughly examined the abdomen, the pelvis is next to be examined by the vagina and rectum, and a conjoined examination of the tumour by the abdomen and pelvis should also be made.

“Examination of the vagina may at once remove all doubt, by showing that the os and cervix uteri are in a healthy state, that the uterus is normally mobile, that its cavity is neither elongated nor shortened, and that any tumour felt through the vaginal wall is independent of the uterus. In such a case the tumour is almost certainly ovarian. On the contrary, we may find the vagina more or less completely obliterated by a solid mass, the cervix uteri gone, the os reached with difficulty, the cervical canal so closed or distorted that the sound cannot be passed, or the uterine cavity so enlarged that the sound may pass many inches beyond the normal length. Here the tumour is almost certainly uterine.

“But it must be remembered that considerable peritoneal outgrowths, or large growths within the walls of the fundus or body of the uterus have been observed, while the uterine cavity has remained unaltered in dimensions and the cervix in structure. And, on the other hand, the cervix may be drawn up out of reach, or the whole uterus may be elongated, when the connexion with an ovarian tumour is close; or the lower portion of an ovarian tumour may be so moulded to the true pelvis that the uterus is pressed upwards and forwards, or flattened behind the pubes, so that the tumour and the uterus are either really or apparently inseparable from one another. Abnormal arterial impulse in the vagina and cervix uteri may be felt in both classes of tumours. But I have never felt the vascular thrill like that of varicose aneurism, occasionally felt in the lower segment of a fibroid uterus, in any ovarian tumour.

“The vaginal walls may be so depressed, when there is much fluid free in the peritoneal cavity surrounding either a uterine or an ovarian tumour, as to form a vaginal rectocele. And the uterus may either remain above the brim of the pelvis if greatly enlarged, or if fixed by adhesion; or it may prolapse with the vagina, the os appearing at the most depending part of the protrusion. Here the uterine sound will generally remove all doubt; for if the dimensions of the uterine cavity are normal, and the weight of the uterus is not increased, the tumour can hardly be uterine. And a uterus which is not much enlarged can generally be pushed up to its normal situation.

“In some cases where the uterus is much elevated, it may be felt through the abdominal wall above the pubes, while the os uteri cannot be reached by the vagina. The urethra may be elongated or drawn to one side, and the bladder may also be displaced. If the abdominal

tumour and the pelvic portion of the tumour fluctuate, while the uterus does not much exceed its normal dimensions, it is almost certain that the uterus is adherent to, and is elevated by, an ovarian tumour.

“Examination by the rectum may show that the uterus preserves its normal size, shape, and position. Or it may be displaced by some tumour above or in front of it, and one or both ovaries may sometimes be felt. This, however, is not very common if they are not enlarged nor lower in the pelvis than usual. By one finger in the rectum and another in the vagina, the consistence, form, and size of any intervening structure can be ascertained, and valuable information so obtained. And if the sound be passed into the uterine cavity, and examination then made by the rectum, it is often easy to ascertain whether any solid or fluid tumour is situated between a normal uterus and the rectum, or whether the uterus is fixed and its posterior part enlarged.

“When a tumour can be felt in the pelvis by vagina and rectum, as well as in the abdomen by the abdominal wall, simultaneous examination will be required to ascertain if there is more than one tumour, and if the uterus is independent or not. Pressing one finger firmly on the cervix uteri, and moving the abdominal tumour with the other hand from side to side, then upwards and downwards, the uterus may be felt to remain almost unaffected by the movements of the tumour, or only to receive some transmitted movement as the pelvic portion of the tumour moves. Here the strong probability is that the tumour is ovarian. On the other hand, every movement of the abdominal tumour may be communicated immediately to the uterus, which is felt to move in all directions with the pelvic portion of the tumour. If this portion is solid, it is almost certain that the tumour is uterine.

“Cases are sometimes met with where ovarian tumours and fibroid tumours of the uterus are both present at the same time. Small uterine fibroids are often observed when the only important tumour is ovarian. I have seen a large cyst of one ovary and a large uterine fibroid co-existing. I have twice seen tumours of both ovaries present when the uterus was enlarged by fibroids, and several cases where both uterus and ovaries were simultaneously affected by malignant disease.

“If these possible complications be borne in mind, such an examination as I have suggested will in most cases suffice to establish an accurate diagnosis between uterine and ovarian tumours. In some cases doubt may still remain, and exploratory puncture or incision may then be necessary.

“If I were to discuss the question whether puncture or incision should be preferred, and what information can be gained by them, I should be compelled to enter into a full account of the physical and chemical characters of the fluids found in ovarian and uterine tumours. I have prepared such an account with much care, and have had many

engravings made to assist in the description ; but this paper has already extended to such a length that I cannot ask for the further attention of the Society now, but hope I may be permitted to bring this part of the subject before the Fellows at some future meeting."

MALIGNANT DISEASES OF THE OVARIES.

Dr. T. Gallard Thomas read a paper on this subject before the New York Academy of Medicine, on 16th March, 1871, and which is reported in the American Journal of Obstetrics for May, 1871. After some remarks on the depreciation of the value of the statistics of ovariectomy, in consequence of the inclusion of operations in which the tumour was not ovarian, and in which the operation should be designated "gastrotomy," and of cases of ovariectomy in which the operation was inappropriate in consequence of erroneous diagnosis, Dr. Thomas enumerates as follows, the cases which have most commonly been confounded with ovarian cyst, and induced a resort to ovariectomy by reason of erroneous diagnosis:—

"Fibrocystic tumours of the uterus ; abdominal dropsy ; colloid degeneration, having for its base the peritonæum and abdominal viscera, the ovaries, or, as I have seen in two cases, the uterus ; and malignant disease of the ovaries. Instances are not wanting in which pregnancy, phantom tumours, uterine fibroids, cystic degeneration of the kidneys, and other conditions have given rise to errors of diagnosis ; but these have rarely done so, while those which I have just enumerated have frequently misled operators of skill and experience. Instances of these affections will often present themselves, in which the most experienced diagnostician will be able to arrive at a positive conclusion only by the aid of paracentesis or an explorative incision, and a certain number will be met with in which, even with these means at his disposal, the most cautious operator will be led into error."

Dr. Thomas quotes Velpeau as stating that "the diagnosis of cancer of the ovaries is extremely difficult: obscure from its inception, assuming various characters, having as its origin different, and for the most part, unascertainable causes. This disease is usually recognized only in *post-mortem* examinations." He then surveys rapidly the existing views of pathologists as to cancer of the ovaries in general, and shows that many cases of multilocular and composite tumours of the ovaries, which we now regard as certainly benign, have been described as malignant in the same way that fibrous tumours of the uterus were thought to be

malignant so lately as the first quarter of the present century, and expresses his concurrence with those authors who teach that a colloid tumour of the ovary, or any other part, may be benign or malignant, as the material that gives it its name develops itself upon a benign or malignant base. The cases that Dr. Thomas regards as really cancerous he classes as follows:—

“1st. The ovary may be affected by true scirrhus degeneration. This form of cancer is decidedly rare, occurs usually in advanced life, and generally creates a tumour not larger than a large orange. It develops slowly, and presents the physical appearance of scirrhus disease in other organs. It may be a primary malignant development, or it may occur in the ovary secondarily, its primary development having been previously recognized in some other parts of the system.

“2nd. The ovary may be the seat of medullary cancerous deposit, which may originate in the vesicles of Degraaf, in a corpus luteum, as Rokitansky once saw it do, or in the stroma of the organ. Distention sometimes causes rupture of the tunica albuginea of the ovary, and then exuberant medullary growth develops in contact with the peritonæum and abdominal viscera.

“3rd. Scirrhus or medullary cancer may alone or united attack the wall of a cyst, and develop either as an endogenous or exogenous production. The cancerous matter so completely invades the cyst-walls in some cases as to make it appear that cystic degeneration had occurred secondarily to its deposit.

“4th. From the wall of a cyst, vascular, arborescent villi may project, lining the cavity, and, in time, filling and distending it so as to cause the rupture of its walls. Then the exuberant cancerous element develops and secrets in immediate contact with the peritonæum, and produces either a dangerous peritonitis or abundant abdominal dropsy. Klob describes this variety of ovarian cancer in these words:

““The arborescent growths projecting from the walls of a cystoid tumour are frequently exceedingly vascular, and often attain to such a development that they completely fill up cysts of considerable size, and after bursting the wall, they project into the peritonæal cavity, whereby, in most cases, extravasation of the cancer-fluid into the peritonæum causes a speedily fatal peritonitis. Rokitansky observed a case in which the originally encysted mass, after passing through the cyst-wall, extended into a neighbouring organ.

““I have seen one case of cystic cancer of the left ovary with arborescent growths which was associated with villous cancer of the body and fundus of the uterus. (In Rokitansky's *Path. Anat. Austalt*, März, 1858, z. 38,944.)

“That form of cancerous growth proceeding from the cyst-walls, especially the villous form, frequently undergoes spontaneous ulceration and softening, which then generally causes speedy death from peritonitis. The carcinoma sometimes spreads from the ovary to the peritonæum, and is then generally associated with abdominal dropsy.’

“With this form of cancer colloid degeneration is often associated, when it constitutes that variety which has been described by Cruveilhier as alveolar cancer.

“The mere presence of villous projections from a cyst-wall must not be regarded as necessarily stamping the growth with malignancy, for it is not rare to see benign papillomatous projections arising from such localities. Dr. Peaslee informs me that fifteen or eighteen years ago he removed an ovarian cyst which was thus studded with arborescent villi, which, at the time, he strongly suspected of malignancy. The patient, however, not only entirely recovered from the operation, but is living at the present time, never having had any development of kindred degeneration elsewhere.”

Dr. Thomas gives five cases, speaking of them as malignant diseases, but not as true cancer. None of them seemed to him to resemble medullary or scirrhus disease as developed in other organs, but had the appearance rather of cauliflower growths of the cervix uteri, and seemed to belong more to the family of vegetating epithelioma.

The first case presented, he says, all the most striking features of malignant disease of the ovary. The development of a solid tumour, soon followed by immense accumulation of abdominal fluid, decadence of strength, cachectic appearance, all coming on in one only 21 years of age, pointed to the probability of malignant disease, but as it was the first case he had met with he did not suspect its nature.

“CASE I. *Malignant Disease of the Right Ovary.*—Mary M., aged 21 years, single, a cook by occupation, was sent to me by her employer on account of an abdominal enlargement. Upon examination I found her very sallow, almost jaundiced in appearance, very large about the abdomen, and exceedingly weak. She informed me that, just one year before that time, she had noticed a slight abdominal enlargement, which had steadily and rapidly increased. At the same time she had undergone rapid emaciation, grown very weak and low-spirited, and now felt that, unless relieved very soon, she would die from exhaustion.

“Physical exploration revealed a large accumulation of fluid in the peritonæum, and, in addition, a round movable tumour occupying the whole of the left side of the abdomen.

"Although the case was not looked upon as a favourable one for operation, it was determined that extirpation should be resorted to. Accordingly, in the presence of Drs. Otis, Swift, Reynolds, Finnell, and Hull, I undertook the operation.

"The patient having been anæsthetized with ether, I cut down slowly through the abdominal walls into the peritonæum. Instantly a large amount of peritonæal fluid escaped, a much greater amount, indeed, than had been computed in the diagnosis. After its escape, I found in the left side of the abdomen a solid mass about the size of a large cabbage, and resembling in feel and appearance an ordinary cauliflower. This was attached to the broad ligament at about the point ordinarily occupied by the ovary. It was too large for removal through the small opening which I had made, but this being enlarged, it was readily turned out. The pedicle, which was short, being secured by two strong hempen ligatures passed through its centre, it was returned, after removal of the growth, to the abdomen, and the abdominal incision closed by interrupted silver sutures."

The patient rallied well after the operation, but always asserted she would surely die. On the seventh day symptoms of septicemia and mania set in, and on the eighth she died. No *post-mortem* examination could be obtained. None of those who had seen the case during life supposed it to be anything but an ovarian cyst, accompanied by a certain amount of abdominal dropsy. The features of the case that should have attracted suspicions were, says Dr. Thomas, the excessive amount of ascites, with a small tumour, the great and rapid enfeeblement of one previously in good health, and the marked appearance of cachexia, but in the second case none of these symptoms existed. Of it Dr. Thomas remarks:—

"While I consider this case as one of true alveolar cancer, I cannot but admit a doubt as to the fact. Considering it as malignant, it presents a very good illustration of the great difficulties which attend diagnosis under these circumstances. Here no abdominal dropsy, cachexia, marked depreciation of strength, or decided emaciation existed; and neither paracentesis nor explorative incision would have thrown light upon the nature of the disease, since all the morbid growth was endogenous.

"Were the counterpart of the first case which I have reported to come before me with my present experience, I feel confident that I should not again err in diagnosis: were the counterpart of the second to present itself, I fear that its nature would escape detection."

In the third case a suspicion of its true nature was entertained,

but not sufficient to authorize one to dissuade the patient from availing herself of the possible advantages of extirpation. The following extract is a description of the tumour after it was removed, with some of the remarks made on this case:—

“Portions of the growths removed were examined microscopically, by Prof. H. B. Sands, who described their appearance thus:

“‘Sections of the tumour, when examined under the microscope, exhibited the following elements: 1. Connective tissue, slightly vascular, arranged in the form of villi, either club-shaped or conical. 2. Flat epithelium, in several layers, covering the villi, but not present in their interior. The appearances observed were those commonly seen in epithelial or cauliflower growth.’

“In this case, although the facility of diagnosis was much impaired by the great thickness of the abdominal walls, it will be seen that a strong suspicion, at least, of the nature of the disease existed. This was not strong enough to make me feel authorized to dissuade the patient from availing herself of the possible advantages of extirpation, and hence I styled the opinion entertained as a ‘suspicion,’ rather than a ‘diagnosis.’

“Uncertain as was the deduction as to the nature of the disease, it may interest my hearers to know upon what it was based. It was not based upon constitutional depreciation or cachexia, for neither of these symptoms existed in such a degree as to be relied upon. It was predicted upon these three facts: first, the great rapidity of development; second, the immense amount of peritonæal effusion, which crowded aside the abdominal viscera so as to cause a disappearance of resonance over the anterior abdominal wall; and third, the existence of two hard masses obscurely discoverable in the iliac fossæ after evacuation of the fluid.

“It may be said that this evidence was not sufficient for a diagnosis. I so fully agree in this view that I have alluded to my deduction from the evidence as a suspicion only.”

In the next case a positive diagnosis of malignant disease was made, and a large medullary cancer of one ovary was afterwards found.

“*CASE IV. Malignant Disease of one Ovary.*—M. G., an unmarried woman, native of Ireland, aged about 24 years, came to my office to consult me for abdominal dropsy. She presented a decidedly cachectic and enfeebled condition, her blood was greatly impoverished, the feet slightly œdematous, and she was very much enlarged about the abdomen. Upon palpation I was struck particularly by two facts: first, that although the sense of fluctuation gave the impression of that superficiality which marks peritonæal effusion, no resonance could be detected

upon percussion over any part of the abdomen when the patient was placed upon the back; second, that in the lower part of the abdomen a tumour, the size, apparently of the head of a child of two years old, could be detected rolling about, and feeling like a pregnant uterus surrounded by ascitic fluid."

In the fifth case a similar diagnosis was made on grounds shown in the following extract, but the result was not determined at the date of publication:—

"In this case the diagnosis, which of course is as yet doubtful, was based upon these facts: here is a lady who becomes rapidly enfeebled and puts on a cachectic appearance at the same time that she discovers a solid tumour over the site of one ovary; this is sensitive, movable, and to my appreciation not attached to the uterus. There is no symptom, either rational or physical, of cirrhosis or other hepatic disease, none of renal or cardiac disease, and none of chronic peritonitis; yet the peritonæal cavity rapidly fills and refills with fluid. The amount of fluid is not moderate, but so great as to crowd aside the intestines (which abdominal accumulation from hepatic disease rarely does), so as to yield to percussion a uniformly dull surface like that yielded by an ovarian cyst. I know of no disease which is so likely to exist, and by its existence explain the phenomena of this case, as malignant disease of the ovary."

Dr. Thomas closes what is one of the most valuable contributions we have had to our knowledge of ovarian disease with the following summary:—

"The circumstances which most prominently point to the development of the disease are:

"1st. The rapid development of a solid tumour in an ovary, with—

"2nd. Marked depreciation of the strength, vital forces, spirits, and general condition of the patient.

"3rd. The occurrence of œdema pedum and spanæmia at an early period, and consequently dependent upon a general blood state, and not the consequence of pressure by the tumour.

"4th. Lancinating and burning pains through the tumour.

"5th. Cachectic appearance.

"6th. The occurrence of ascites without evidence of cirrhosis or other hepatic disease, organic disease of the kidneys, or heart, or chronic peritonitis, the fluid accumulating in such large amounts as to force aside the supernatant intestines, and produce dulness in place of resonance on percussion in dorsal decubitus.

"Cystic degeneration of the ovary sometimes advances with great rapidity, and is accompanied in its course by rapid emaciation, marked

physical prostration, ascites, and a cachectic appearance. It may be asked whether a case thus complicated would not present the very conditions which have been pointed out in this essay, as furnishing grounds for the diagnosis of malignant disease. Unquestionably it would; but let it be remembered that while these symptoms are mentioned as valuable aids to diagnosis, I do not pretend to maintain that they will always enable the diagnostician to avoid error. Again, in citing ascites with a small tumour as a most important symptom of malignant ovarian disease, I do not allude to slight or even moderate effusion with a large growth, but a markedly disproportionate amount of fluid, a great deal of abdominal effusion with a very small tumour.

“Besides the condition just mentioned, there are two others which may create difficulty in differentiation from ovarian cancer,—one is pregnancy in the middle or latter months, complicated by peritonæal effusion; the other a uterine fibroid existing with cirrhosis of the liver, with its attendant dropsy. The first may generally be known by its characteristic symptoms; while the second, although it might be recognized by the physical and rational signs of uterine fibroids and of cirrhosis, would very likely give considerable trouble in diagnosis.

“When difficult and obscure cases present themselves in which a positive diagnosis becomes impossible by ordinary means, paracentesis or explorative incision should be resorted to, rather than that the patient should be deprived of the prospect for cure held out to her by ovariectomy. Very often the most doubtful case may be satisfactorily settled by evacuating the abdominal effusion, and passing the index finger through a small opening in the peritonæum, so as to touch the morbid growth. In certain rare cases, such an one, for example, as Case II. of this essay, even this would not suffice to remove all doubt.

“In concluding this paper, it may not be without interest to allude more particularly to the great amount of abdominal effusion which, in cases of ovarian cancer, constitutes such an important symptom. It is probably to a certain degree the result of peritonæal secretion, stimulated to excess by the irritation established by contact with the exuberant morbid growth, which, having burst its ovarian bounds, crops out into the peritonæal cavity. To a certain extent the accumulation is probably also due to a secretion from the free surface of the cancer itself.”

There are, however, other conditions in which there may be a small movable tumour with a large quantity of fluid in the cavity of the peritonæum, to which Dr. Lee does not allude. One is where there is a tumour formed of an aggregation of cysts, one of which gives way, and not only empties itself into the peritonæum, but continues to pour out fresh secretions till the peritonæum is

fully distended. The present reporter has recorded a case of this kind in the Proceedings of the Dublin Pathological Society, published in this journal, August, 1870, p. 199. In this case the fluid drawn from the peritonæum had the thick mucilaginous character of that secreted by the ovary, and this circumstance would probably lead to a correct diagnosis. Mr. Spencer Wells has described another condition in which there was a moderate sized tumour with a great quantity of ascitic fluid. The case was recognized as one of fibroid tumour, and Mr. Wells removed it successfully:—

“A married French dressmaker, aged 46, was admitted on June 6, 1871, in a condition of extreme suffering—indeed, almost moribund. Her abdomen was enormously distended, measuring fifty-three inches in circumference—thirty-two from sternum to pubes, and thirty-eight across from one anterior superior spine of ilium to the other. Her legs were hard and œdematous, and there was a large vaginal rectocle, although the uterus was kept up by a stem and inflated pessary. Mr. Wells tapped immediately after admission, and removed fifty-nine pints of clear straw-coloured fluid from the peritonæal cavity. A hard, nodulated, movable tumour could then be felt, filling the lower part of the abdomen, extending four inches above the umbilicus, and with a partially separable nodule, which reached the false ribs on the left side. The vaginal rectocle was not much diminished by the removal of the fluid. The cervix uteri was high and movable, and only slightly affected by the movements of the tumour.

“Considerable relief was afforded by the removal of the fluid, but it began to accumulate again very fast, and her condition became so pressing that Mr. Wells removed the tumour two days before the usual operating day. An incision was made in the median line from above the umbilicus to two inches above the pubes; it was about nine inches long. Six pints of ascitic fluid escaped, and a solid tumour was exposed, springing from the right side and back part of the fundus uteri. The chain of a large écraseur was tightened slowly around the bridge of connecting tissue between the tumour and the uterus, and the tumour was cut away and then removed, after separating large shreds of adhering vascular omentum.”^a

MANAGEMENT OF THE OBSTETRICAL FORCEPS.

Dr. Clarke, of Oswego, has contributed a very vigorous paper on this subject, that is quoted in the *American Journal of Obstetrics*, for May 7. The following are the points touched upon, in all of

^a Fibroid Overgrowths from the Uterus—Ascites—Removal of Fluid and Tumour—Recovery.—*Medical Times and Gazette*, p. 129, July, 1871.

which Dr. Clarke thinks he differs more or less widely from the received authorities; and certainly he does differ from the theoretical directions of most authorities, but not so much, probably, as he thinks from their actual practice. For, as Barnes remarks, Dr. Ramsbotham confesses he purposely violates the rules laid down for applying the instrument in accordance with the position of the head, and many others do the same thing without knowing it:—

“The points that I propose to touch upon are—

“I. The danger that attends the use of the forceps.

“II. The exigencies that call for the forceps.

“III. The best kind of forceps.

“IV. The position of the patient.

“V. The law of application.

“VI. The manner of introduction.

“VII. Locking.

“VIII. Slipping.

“IX. Compression of the head.

“X. Management in extraction.”

Under the first of these heads Dr. Clarke quotes freely from various sources the dangers said to attend on the use of the forceps, and says these terrible representations seem to him ridiculous and absurd. The forceps, he affirms, is not in any material degree a dangerous instrument to the mother—though it may not be as harmless to the child as to the mother. The features of the infant are often temporarily and sometimes permanently injured, while the bones and even the viscera of the head are sometimes fatally crushed, but these evils, he asserts, and most truthfully, are entirely unnecessary, and proceed solely from the want of skill of the operator; and all must agree with him when he says, in another place, the obstetrician, whose instrument disfigures the new-comer, is a bungler, whose only excuse is that he was taught in a bad school.

As to the exigencies that call for the forceps, Dr. Clarke's remarks may be quoted in full:—

“II. *What are the exigencies that call for the forceps?* This question in my opinion admits of a simple and comprehensive answer. The occasion, the justification, and the obligation of using this instrument are co-extensive and identical. Whenever, in a head presentation, with probable room for the head to traverse the pelvis, and with the os fully dilated or partly dilated and easily dilatable, the longer continuance of unaided

labour involves danger either to the mother or to the child, or even a longer duration of suffering to the mother, the forceps should be used. I go further; I hold that the forceps is justifiable sometimes in order to cut short the unnecessary protraction of those anxieties of the patient and her friends, that attend uncompleted labour, and even to save the time of the practitioner himself. I am well aware that in this opinion I widely depart from the maxim that authority sustains. Churchill, though reckoned an advocate and defender of the forceps, lays down the rule, in italics, for emphasis, that ‘they are to be applied in no case, till we are perfectly satisfied that the obstacle cannot be overcome by the natural powers with safety to the mother and the child.’ It is such a rule as this, causing perilous delay, that makes this instrument, in crude statistical tables, seem the means of death. I repudiate this rule. It is a rule that may fitly be followed by those who believe the operation to be difficult or dangerous, but it is not one for me, who think I find it as easy as the passing of a catheter, and as innocent as giving an enema.

“As it is not my purpose to write a systematic treatise on the forceps, but only to touch upon those matters where I think the existing practice is erroneous, I shall not enumerate and discuss in detail the many items that are comprehended in the rule that I have laid down, but shall content myself with some observations on certain supposed limitations of it that are to be found in the text-books. Those limitations that hang on the supposed dangerousness of the operation I have already sufficiently discussed.

“It is said that great violence of the pains contraindicates the forceps, on the ground that a reinforcement of the expulsive power would be dangerous. This doctrine is entirely erroneous. The use of the forceps in such cases, in addition to its ordinary advantages, saves the womb from some part of that perilous violence of muscular action that, besides minor evils, sometimes threatens its own integrity.

“Neither is extreme resistance or rigidity of the soft parts a contra-indication. It is even an indication. If rigid perinæal tissues be the obstacle, the danger of their laceration will be lessened by the forceps. The wedge-like form of the proximal end of the locked blades is an important aid in dilatation. It prepares the way. Meantime it diffuses the bearing of the uterine force along the longitude of the vagina, lessening its intensity at any point. On the other hand, so far as the resistance is due to the action of the perinæal muscles, greater mechanical force in overcoming it is no way objectionable; and it can certainly be more cheaply furnished by the arm of the obstetrician than by the uterus of the mother.

“Moreover, the experienced practitioner will remember that a majority of the cases of laceration of the perinæum occur when, after long delay at that point, and many ineffectual pains, the uterus, as if vexed with the

futility of its efforts, with one tremendous throe suddenly bursts through the obstacle. Reflex power, when repeatedly foiled, does thus accumulate. The forceps, by securing the steady progress of the head, in some degree obviates the danger.

“Besides this, it is to be remembered that laceration of the perinæum seldom or never occurs, save when there is a congenital deficiency of the elastic tissues of the vulva. This imperfect development may be hardly appreciable, or it may approach atresia. If it exist in even a slight degree, laceration of the parts is perhaps inevitable. Whatever the degree of danger may be, it will not be increased by the forceps unless the final delivery be wantonly and violently precipitated. The additional bulk made by the blades is next to nothing, and is more than balanced by the slight elongation of the head that almost necessarily attends their use. Meantime their wedge-like shape, beginning earlier the dilatation of the external parts, necessarily makes it more gradual and therefore safer.

“Not a few authors declare that the use of the forceps should not be attempted when the head is above the superior strait. I can conceive of no good reason for this limitation. The operation under these circumstances is somewhat more difficult to the inexpert, and is sometimes impossible, but it is entirely free from the objection of peril. When the waters have escaped, tonic uterine contraction generally holds the head firmly against the inlet of the pelvis, with a larger or smaller segment of it engaged therein. In this condition a tolerably expert operator will have no great difficulty in grasping it with his instrument, and, if it be not detained by insuperable mechanical obstacles, delivery can be readily effected. On the other hand, when, from deficient tonicity of the uterine walls, or from the presence of a considerable quantity of amniotic fluid, or from both of these causes combined, the head is freely movable or floats above the pelvic brim, the attempt to apply the forceps will be of doubtful success. Turning is then the surer resource. This exigency may co-exist with hæmorrhage, convulsions, or other accidents calling for speedy delivery; but it can hardly obtain in any of the forms of dystocia proper, save in that in which there is considerable narrowing of the pelvic brim as to make impossible any other means of delivery than embryotomy. The principal source, in my opinion, of the objection that many authors entertain to the use of the forceps when the head is in high situations, is in their utterly erroneous mode of applying the instrument. It is this that makes very many respectable authors oppose its use even when the head is already engaged in the superior strait, and needs but the touch of skill to cause it to finish its course.”

Dr. Clarke prefers the long, double-coursed forceps. To the possessor of this instrument the short forceps is, he says, utterly useless:—

“Every thing that can be done with the latter can be done with equal facility, safety, and painlessness with the former. The obstetrician who sports both kinds, must be of a piece with the well-known gentleman who directed his carpenter to make in his garret-door a big hole for the old cat and a little one for the kittens!”

As to the position of the patient, Dr. Clarke thinks the ordinary obstetric position on the left side has certain advantages; but he prefers having the woman upon her back, and recommends this position; but it is on the next head, or principle of application, Dr. Clarke lays most stress, and it is here he thinks that he differs most materially from the American, English, and French authors that happen to be within his reach. The grand question in the use of the obstetrical forceps is, he says, whether the law of their application should have relation to the particular presentation of the head or the curve of the pelvic axis; to the anatomy of the child, or to the anatomy of the mother. Almost all the authorities teach, he says, that it is the position of the head that should determine the position of the blades; but this rule is, in his opinion, entirely erroneous. Sir James Simpson, he thinks, was “partly aware of the absurdity of this rule.” Dr. Clarke considers this rule not only an error, but a very grave error, and one that leads to very grave results. He holds that the blades should simply follow the course of the utero vaginal canal, and when applied should in all cases be in accord with the course of the pelvic axis, regardless of the presentation. Wherever the sides of the head may be, the blades should be applied to the sides of the pelvis:—

“I assert, that till the head is actually at the outlet of the pelvis it is substantially impossible to apply the forceps in any other than the manner I have indicated. A slight deviation of the instrument toward an oblique diameter I admit to be possible, but its own shape and the laws of mechanics confine that deviation within narrow limits. How can you place the blades along the parietal bones when the plane of those bones makes an angle with that part of the pelvic axis in which the head is situated? Or how can you, without undue violence, lay them there when their pelvic curve must widely divert from and antagonize the curve of the maternal passage? The curve of the vagina still exists even when that canal is dilated to permit the passage of the head: can it be disregarded in the mechanics of forceps delivery? If we compare the distance, following the sacral curve, between the posterior commissure of the vulva and the posterior edge of the pelvic brim, with its anterior counterpart, it will be obvious that the blades, one following one line and

the other the other line, cannot be brought squarely and symmetrically to embrace the head, without forcing their handles violently back to the very coccyx. Nor, when the head is at the superior strait, can it be done even thus. Nevertheless, these are the virtual impossibilities that authors and lecturers, in the most matter-of-course way, call upon us to perform. They hardly ever suggest a difficulty or a doubt. Their language would make one think that the forceps can be played about in the female pelvis, with its pelvic curve bulging this way or that, as freely and easily as in an indian-rubber bag or in a barrel.

“To cap the climax of absurdity, our professors illustrate their instructions on that most useless and preposterous of all human contrivances, called by Dr. Meigs, with unconscious appropriateness, ‘the Phantom.’ I well remember, as a pupil, spending hours over that effigy, learning, as I innocently supposed, to apply the forceps to the sides of the head when it presented in this, that, and the other position. Nothing could be less like nature, and nothing, therefore, could be less instructive. It was like breakfasting on the morning fog. You might as well practice passing a catheter on the town pump.”

If Dr. Clarke used the straight forceps instead of a curved one, he would not attach so much importance to this matter, or write so excitedly on it. It is one of the advantages of this instrument that it adapts itself to the pelvis easily and safely. Held as lightly as a feather in the operator's hand, it finds its way into that part of the pelvis where there is most room, it presents no curves to get into opposition with the curves of the passage it has to traverse. It has no projecting point to endanger the urethra or the other parts lying in the pelvis. It does not require the careful guiding so elaborately described by Dr. Clarke as necessary in the use of the curved instrument. It may be applied in the oblique, the transverse, or the antero-posterior diameter, if the necessity should arise, and if in its passage through the pelvis the head tends to turn from the third or fourth to the first or second position, the straight instrument turns with it, and yet does not endanger the soft parts, or require to be taken off and re-applied.

Dr. Clarke protests against the use of the forceps for compressing the head, showing that any influence it could exert in this way would tend to increase the difficulty of extraction. He uses the instrument simply as an extractor, and objects to the lateral motion, so much talked about in systematic treatises:—

“X. *Extraction.*—It would hardly seem to admit of dispute that the extractive power of the forceps ought to be used in imitation of nature,

and, accordingly, in the direction of the expulsive action of the uterine and abdominal muscles as modified by the lines of the pelvic passage. Nevertheless, for some incomprehensible reason, we are told by Prof. Bedford, that the force exerted by the obstetrician should be 'one third extractive and two thirds lateral.' This is also substantially the advice given by Prof. Meigs and other popular authors on midwifery. Such management of the forceps is not in imitation of nature. Nature does not seesaw or wriggle her burden along. Were the walls of the passage as dry, friable, and in-elastic as those of a port-hole, or were the foetal head so rough and angular as to readily secure a bearing on those walls, this *prying* it out would not be unreasonable; but in reality its oval and gliding surface cannot be hastened along its lubricated and elastic road by working it from side to side. True it is, that, if our first efforts at moving the head along fail of success, we may very properly direct our subsequent tractions, tentatively, a little this way and that, distrusting the correctness of our judgment as to the exact law of the case, and thus learn to aid aright the vis-a-tergo. With this exception, based on the imperfection of human judgment, traction is the only function of the forceps."

CÆSAREAN SECTION.

Another fatal case of this operation has been recorded.* The patient was a dwarf 3 feet 6 inches in height, at the full period of pregnancy. She was two days in labour before Dr. Inglis saw her, but her condition was probably as good as can be expected in such cases:—

"On my arrival I found the pulse pretty good, but was told that it flagged considerably at times. The skin was in good condition and the tongue moist, while there was little abdominal tenderness. The vagina was cool and moist, with the usual viscid mucous secretion tinged with meconium; and the os was flaccid and dilatable. The breech was attempting to present, but the inlet of the pelvis was so small that only a portion of the nates could be felt, the cedematous scrotum of the child nearly filling the pelvis."

The operation was remarkable in two particulars—1st. The incision was made at a considerable distance from the mesial line, so as to get into the thickness of the rectus muscle. Dr. Inglis thinks the parts are so thinned at the mesial line by the distention of pregnancy, that it is difficult to secure perfect apposition, and the sutures are liable to tear out. 2nd. The uterine incision was

* A Case of Deformity of the Pelvis in which Cæsarean Section was performed. By A. Inglis, M.D., Prof. Midwifery, Aberdeen.—Edin. Med. Jour., Oct., 1871.

made "within the circle of the muscular fibres that surround the Fallopian tube," that is curvilinearly, so as to avoid as much as possible dividing the muscular fibres. This mode of incision Dr. Inglis claims as original.

The woman died on the third day. The fate of the child is not recorded.

TRANSFUSION.*

In the Transactions of the Dublin Obstetrical Society in the present number of this journal, there will be found a remarkable discussion on transfusion, and it seems important to direct attention here to Mr. Higginson's paper. Of the thirteen cases ten were connected with parturition or pregnancy. In five the operation was done on account of *post-partum* hæmorrhage. In three on account of placenta prævia. In one there was probably an abortion at the fifth month; and in one an extra uterine pregnancy with rupture of the cyst. In one case blood was transfused because of hæmorrhage from an erysipelatous arm, and the patient recovered. In one the operation was called for by extreme prostration from prolonged suckling. This patient also recovered. In another there was collapse from acute mania; the patient improved for a time, but died in forty hours from disease of brain and lungs. Of the five *post-partum* hæmorrhage cases three of the patients recovered. One died seven days after operation, and the uterus was found internally offensive and purulent. In one there was considerable delay before the operation could be adopted, and the patient died almost immediately on the injection of about 7 oz. of blood.

In three cases the hæmorrhage arose from placenta prævia. In one the patient improved, but the hæmorrhage returned, and she died in three hours. In one the pipe was jerked out of the vein when about 8 oz. had been injected, and she died undelivered. In the remaining case the supply of blood was very bad, and about 12 oz. of salt and water were injected with some improvement. The patient was delivered and died. In the abortion case the patient made a good recovery, and in the extra uterine fetation case the vein opened was very obscure, a large thrombus resulted from the injection, and too little entered the system to do any good.

* Tabular Report of Thirteen Cases of Transfusion of Blood, with Diagram of the Operator's Instrument, and Observations by Alfred Higginson, M.R.C.S., Consulting Surgeon to the Liverpool Southern Hospital.—Liverpool Medical and Surgical Reports, 1871, p. 104.

In all of the thirteen cases blood, fresh from the arm, without admixture or manipulation, was employed. The largest quantity injected was 20 oz., and the smallest 4 oz., the average of all the cases about 9 oz. The instrument used was a special one, essentially a "Higginson's syringe" reversed, and a funnel added to receive the blood. The objects aimed at were to avoid the introduction of air, to keep the blood at an even temperature, and always moving onward. An average of 9 oz. in each case before coagulation shows, in Mr. Higginson's opinion, a sufficient capability of practical utility with pure blood. Still, he says, the experiments with defibrinized blood, or with solutions added to blood, may be ultimately of value in practice.

PART IV.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-FOURTH SESSION.

DR. KIDD, President.

An Address, delivered on the 18th November, 1871, at the Opening Meeting of the 34th Annual Session of the Dublin Obstetrical Society, by the President for the past Session, GEORGE H. KIDD, M.D., F.R.C.S.I.; Hon. Fellow of the London Obstetrical Society; Corresponding Member of the Gynæcological Society of Boston; Obstetric Surgeon to the Coombe Lying-in Hospital.

GENTLEMEN,—It now becomes my duty and my privilege to address you, partly by way of summing up the work of our Society for the past Session, and partly to offer suggestions as to our future. We enter this evening on our thirty-fourth annual session, and I am happy to say we are, in every respect, in a most flourishing condition. We have a goodly roll of members; our finances are in such a condition as to allow your council to think themselves justified in undertaking that which they have long desired—the publication of an annual volume containing a full report of the papers read at our meetings, and of the discussions that take place thereon, and our proceedings indicate vigour, progress, and good and faithful work.

During the past session we have had papers on the three subjects for the cultivation of which our Society was originally founded. On pure midwifery we have had six communications. Dr. George Johnston, the present Master of the Rotunda Hospital, read a clinical report of the practice, for the year 1870, of that great institution, a paper of rare value and importance. From the same hospital we have had another paper, also of great interest, contributed by Dr. T. More Madden, and since published in the *American Journal of Obstetrics*, “On Sudden Death after Labour.” At our March meeting, Dr. M’Clintock, to whom it was sent for the purpose, read a paper from the pen of Dr. J. Matthews Duncan, of Edinburgh, “On the Efficient Powers of Parturition;” and, at the February meeting, I occupied your attention myself with a memoir “On Decapitation as a Mode of Delivery in Cases of Shoulder Presentation, in which Version cannot be safely effected.” At our last meeting Dr. Byrne exhibited a remarkable substance, expelled from the

uterus before labour, and also a portion of membranes retained in the uterus for a considerable time after labour. Earlier in the session, we had before us a very interesting specimen, now in the Museum of the College of Surgeons, of extra uterine fetation, in which the progress of the ovum was arrested in the Fallopian tube, close to its fimbriated extremity, where it grew till it ruptured its cyst, and caused the death of the mother by hæmorrhage into the cavity of the peritonæum.

In papers on the diseases of women, our proceedings have been specially rich; Dr. Atthill read one "On Some Forms of Enlargement of the Uterus," in which he traced, in an exhaustive manner, the causes that give rise to this condition. Dr. Byrne related a case of vaginal tumour and prolapse cured by an operation; and we had two papers on menorrhagia:—one by Dr. Gogarty, who related a case in which the hæmorrhage depended on a fibrous tumour developed in the uterine walls, and was stopped by dilatation and a free application of fuming nitric acid; and one by Dr. Atthill, in which the various forms of this condition were discussed, and especially with reference to treatment. At the January meeting I brought another form of hæmorrhage, from the non-gravid uterus, before the Society, and exhibited a specimen of epithelioma, removed, on the preceding 5th December, from the uterus of a lady, whose health was rapidly sinking from constant bleeding, and who, I may now, perhaps, be allowed to state, has since remained in perfect health; and at subsequent meetings, I exhibited several large fibrous tumours, removed from the interior of the uterus by *écrasement* with a single steel wire, the principal symptoms of which had been hæmorrhage.

On the diseases of children we had two communications, one of great practical importance, from Dr. M'Swiney, "On the Green Stools of Infants;" and one from Dr. Roe, who exhibited a specimen of that form of malformation known as a Cyclops.

It seems to me that societies such as ours are working best, and exercising the highest functions belonging to them, when their proceedings reflect the progress being made in the departments of knowledge for the cultivation of which they were founded, and, at the same time, add their share to this progress. Judged by this standard our Society is working well, and this, I hold, to be no mean praise, for in no department of medicine has more rapid and gratifying progress been recently made than in that of obstetrics. I propose now to invite your attention to a brief examination of some of the subjects in which this progress has been most marked. To many I see around me I know this will be as a twice-told tale, for they have been the actors in much of what I have to relate; but I must throw myself on their indulgence for the sake of bringing before our brethren who are not so immediately connected with our branch of the profession, an outline of what we are doing, whereby we may not only strengthen the ties that bind us

together in the pursuit of the one great cause—the improvement of the science, and, consequently, of the art of medicine—but, perhaps, also receive suggestions from some who see things from a point of view different from our own.

USE OF THE FORCEPS IN TEDIOUS AND DIFFICULT LABOURS.

“There is no subject connected with the practice of midwifery,” says Dr. Collins, “so difficult to acquire a sound knowledge of as the treatment of tedious and difficult labours. It is one,” he says, “of the most vital importance, and in the most marked manner distinguishes the experienced from the inexperienced practitioner.” Fully appreciating the force and truth of Dr. Collins’ remark, I shall take up, in the first place, this vitally important subject—the treatment of tedious and difficult labours—adopting as my starting point the reports of the Rotunda Lying-in Hospital, read by Dr. Johnston, during the past and preceding sessions. These reports give us the clinical history of the hospital for two years. They were written mainly to illustrate the history of puerperal fever, and do not go as fully as I could wish into some of the matters to which I have to refer; but Dr. Johnston has not only supplied me with all the details I required, so as to enable me to make the inquiry I am entering on a full and searching one, but he has given me the corresponding particulars of the practice of the hospital for the year ending on the 5th inst., which have not yet been published, and we shall consequently have before us the practice of the three years he has been master.

During the three years there were 3,338 women delivered in the hospital, in addition to 69 cases of abortion. Of the 3,338 women 227 were assisted with the forceps, being at the rate of 1 in 14.74. Now let us compare this with the history of the same hospital so far as published records may enable us to do so. The first published report of the hospital is that of Dr. Joseph Clarke, who states that he used the forceps once in every 728 cases, and his biographer further states that he only used it “once in the multitude of cases under his care in private.” Dr. Clarke was master of the hospital from 1787 to 1793, and he speaks of “extractive instruments” having been in common use in this country up to his time. His teaching, however, raised a prejudice against them, which, in a short time, became so determined that a gentleman who held the office of master some time after him, and who, for very many years, enjoyed one of the largest obstetric practices in this city, stated, at a meeting of this Society in 1842, shortly before his retirement from the active duties of his profession, that he had “once tried the forceps and failed, and would never try it again.” Here the tide of opinion seems to have turned, for we find that Dr. Collins, who was master of the hospital some time after this gentleman, used the instrument once in

608 deliveries; and Dr. Charles Johnson used it once in 106·88 deliveries; Dr. Shekleton once in 68·74; and now we have Dr. George Johnston using it once in 14·74. But it may be asked is this progress in the right direction, or is it only a further development of the "meddlesome midwifery," and of the practices of the "obstetric reprobates," against which we have all been so often and so eloquently warned. This may be best answered by comparing the results obtained in the hospital under the care of each of these masters. (See Table, p. 68.)

In making this comparison it is necessary to confine it to the results obtained in the treatment of the special class of labours in which the question as to the propriety of using the forceps arises. It is obvious that to compare a great number of heterogeneous cases, as has been done, for instance, in the last edition of Murphy's Midwifery, could be of no use. I shall, therefore, confine my comparison to cases of tedious and difficult labour, and in making it, I shall divide the reports into two groups. In the three earlier reports, that part of Denman's definition of difficult labour, that it shall exceed twenty-four hours in duration, is strictly adhered to, whereas in the two later reports time is not recognized as a part of the definition. Dr. Shekleton and Dr. Geo. Johnston were guided in considering the necessity for operative interference by the condition of the mother, quite independently of the length of time she had been in labour. But as Dr. Geo. Johnston, who has given us the last report, was one of Dr. Shekleton's assistants, and drew up, conjointly with Dr. Sinclair, the report of Dr. Shekleton's mastership, we may safely regard the personal link which thus connects these reports, as justifying the comparison, even though definitions be not so strictly adhered to.

In Dr. Clarke's report, 183 cases of tedious and difficult labour are recorded; of these he allowed more than 66 per cent. to be delivered without assistance. He used the forceps in 6·55 per cent., and the perforator in 26·78, and 20·21 per cent. of the mothers died. Dr. Collins met with 210 cases, but he gave assistance more readily than Dr. Clarke. Instead of 66·67 per cent. delivered without aid, he only allowed 55·72 to go on unassisted, and instead of losing 20·21 per cent. of the mothers, he only lost 14·76 per cent. The increased assistance, however, was with the perforator. He used the forceps very little more frequently than did Dr. Clarke, viz.: in 6·66 per cent.; but he used the perforator in 37·62 per cent. instead of 26·78. Dr. Chas. Johnson did not give assistance so frequently as Dr. Collins. His percentage of unassisted cases was nearly the same as Dr. Clarke's, 66·80; but, he used the forceps twice as often as either, viz.: in 13·13 per cent., and did not use the perforator in more than 20·07 per cent., instead of the 37·62 of Collins, and the mortality of the mothers was not more than 8·49 per cent., instead of the 20 and 14 per cent. of Clarke and Collins.

TEDIOUS AND DIFFICULT LABOURS.

	CLARKE 1786 to 1793		COLLINS 1826 to 1833		C. JOHNSON 1840 to 1847		SHEKLETON 1847 to 1854		G. JOHNSTON 1868 to 1871	
	Actual number observed in 10,199 Labours	Per Cent.	Actual number observed in 15,850 Labours	Per Cent.	Actual number observed in 6,634 Labours	Per Cent.	Actual number observed in 13,748 Labours	Per Cent.	Actual number observed in 3,338 Labours	Per Cent.
Total number of tedious and difficult labours, -	183	1.79	210	1.32	259	3.90	514	3.73	271	8.12
Of which were delivered unassisted, -	122	66.67	117	55.72	173	66.80	247	48.05	55	20.30
" " by forceps or vectis, -	12	6.55	14	6.66	34	13.13	168	32.69	204	75.28
" " by perforator, -	49	26.78	79	37.62	52	20.07	99	19.26	12	4.42
Deaths of Mothers : -										
Total after tedious and difficult labours, -	37	20.21	31	14.76	22	8.49	31	6.03	20	7.38
" delivered unassisted, -	15	12.29	12	10.25	9	5.20	12	4.85	4	7.27
" " by forceps or vectis, -	6	50.0	4	28.57	5	14.70	6	3.57	14	6.86
" " by perforator, -	16	32.65	15	18.98	8	15.38	13	13.13	2	16.66
Deaths of Children : -										
Total after tedious and difficult labours, -	97	53.005	113	53.809	120	46.33	168	32.68	26	9.59
" delivered unassisted, -	42	34.43	29	16.95	52	30.05	49	19.83	6	10.90
" " by forceps or vectis, -	6	50.0	5	35.71	17	50.0	20	11.90	10	4.90
" " by perforator, -	49	100.0	79	100.0	52	100.0	99	100.0	12	100.0
Total forceps or vectis cases, -	14	—	27	—	42	—	226	—	227	—

When we compare the reports of Dr. Shekleton's mastership and of Dr. Geo. Johnston's, we find a still greater increase in the frequency with which the forceps is used, and while the maternal mortality is 1·35 per cent. greater in Dr. Geo. Johnston's report than in Dr. Shekleton's, there is a very marked decrease in that of the children. Dr. Shekleton allowed 48·05 per cent. of his tedious and difficult cases to be delivered without assistance. Dr. Geo. Johnston only 20·30. Dr. Shekleton used the forceps in 32·69, Dr. Geo. Johnston in 75·28 per cent. Dr. Shekleton used the perforator in 19·26, and Dr. Geo. Johnston in only 4·42 per cent. of his cases. Dr. Shekleton lost 6·03 per cent. of the mothers after tedious and difficult labours, Dr. Geo. Johnston 7·38. It is evident thus, that Dr. Geo. Johnston used the forceps in very many cases in which Dr. Shekleton would have used the perforator, and it seems probable that the increased maternal mortality is due to this cause, and the fact that the perforator was only used in 4·42 per cent. of the cases, and that the mortality rose to 16·66 per cent., shows that this instrument was used only in very extreme cases. Taking the whole five reports however, the total mortality of the mothers after tedious and difficult labours ranges as follows:—20·21, 14·76, 8·49, 6·03, 7·38 per cent., and of those delivered with the forceps 50 per cent., 28·57, 14·70, 3·57, 6·86 per cent.; and this I claim as a very important and marked progress in the right direction. Again, of the whole number of children born after tedious and difficult labours, Dr. Clarke lost 53·005 per cent., Dr. Collins 53·809, Dr. Charles Johnson 46·33, Dr. Shekleton 32·68, and Dr. Geo. Johnston 9·59 per cent. Of those born by the aid of the forceps, Dr. Clarke lost 50 per cent., Dr. Collins 35·71, Dr. Charles Johnson 50, Dr. Shekleton 11·90, and Dr. Geo. Johnston 4·90. In a large proportion of Dr. Charles Johnson's cases, the children died from the use of ergot of rye, a remark that applies also, though in a less degree, to Dr. Shekleton's cases, but notwithstanding this we have here demonstrated a very important saving of infant life—Dr. Clarke's loss being 53·005, and Dr. Geo. Johnston's 9·59—in addition to the saving of the mother's lives, in consequence of the early use of the forceps—a further proof of progress.

Progress is generally so gradual in its growth that it is impossible to say where it begins, and what are the stages of its development; but in the present instance it is not so. We have seen that it was the teaching of Dr. Clarke that almost entirely banished the forceps from the armamentarium of the Irish Obstetrician; but, great as was his authority and influence, there was found one among his pupils to protest against the erroneous teaching of his master, Dr. John Beatty, Dr. Clarke's first assistant in the hospital. Since then the use of the forceps has gradually become general in Dublin, to which the influence and example of Dr. Thomas Edward Beatty and Dr. Churchill, have much contributed, for not only did they teach the use of the instrument in the hospitals under

their care and in their writings, but by their improvements in its construction they rendered its application more easy and safe. In the Coombe Hospital the same principles have long been taught, and we all rejoice now to find the great institution to which the Dublin School of Midwifery owes so much of its reputation, the Rotunda Hospital, taking its rightful position in the van of the movement.

In private practice, I believe, the forceps is now used with pretty much the same frequency as in the hospitals. In my own practice I find I have during the last seven years, in cases that have been under my own care from the beginning of labour, used it at the rate of once in every sixteen cases. In my earlier career I did not use it nearly so frequently, but I believe the results I have obtained fully justify the change.

During these seven years I have never lost a mother from tedious and difficult labour, where the case has been under my own care from the beginning, nor has there been one child still-born that did not show indisputable evidence of having been dead some time before labour began. During these seven years I have only given ergot of rye to hasten labour to one patient; and I have never used the forceps without having the approval, presence, and assistance at the operation of another practitioner; for however I may differ from Dr. Clarke and Dr. Collins in other matters, I quite agree with them in this—that no operation should be undertaken in midwifery without a previous consultation, when it can possibly be obtained. During the same seven years I have performed craniotomy but once, on a patient who had been under my own individual care from the commencement of labour, and then merely to save the mother from a prolonged labour, when the child was known to be dead. It would be difficult for me to describe the pleasure it affords me to look from time to time on children now growing up who were born by the aid of the forceps, but who so recently even as when I was a student would have been the subject of craniotomy.

I do not mean to enter at present into the consideration of the arguments that have been adduced against the use of the forceps, but there is one that I cannot pass over, that the use of the forceps is the cause of vesico-vaginal fistula. I believe the truth of the matter is that it is the *non-use* of the forceps that is the cause of this sad accident, almost the greatest misfortune that could happen to a woman. This has been demonstrated recently in a remarkable manner by Dr. Busey, of Washington, in a paper published in the August number of the *American Journal of Obstetrics*. Dr. Busey analysed the cases recorded by Dr. Emmett in his book on Vesico-Vaginal Fistula, and asserts that “the lesson taught by this analysis is that impaction is the usual cause, and that delay in resorting to artificial means to expedite delivery after it has occurred, incurs not only the danger to the mother, but imperils the life of the child. Of these 65 cases 50 children were certainly lost. Though

instruments were employed in very many of the cases, it is perfectly apparent that the error was in not having resorted to artificial means sooner. Mr. Baker Brown has also shown that protracted labour is the almost invariable cause of fistula, and he advises the acceleration of labour as the best means of preventing it.

IMPROVEMENTS IN EMBRYOTOMY.

But though the use of the perforator and the frequency of embryotomy has been gradually reduced to a minimum, this very circumstance instead of causing the operation to be neglected has led to its improvement. Embryotomy is now only performed in extreme cases, and consequently the instruments that served well enough in cases of little difficulty soon had their inefficiency made manifest when they came to be used only in cases of great deformity. The improvement of the instruments and of the methods of operating therefore became a necessity, and to this object have been directed the attention of obstetricians in all lands. In Italy and in France, in Austria and in Belgium, in America, and at home, improvements have been suggested. Long before the necessity for it was recognized elsewhere the cephalotribe was invented by Assalini, to whose genius we are indebted for many other improvements, for the extraction of the head in cases where the conjugate diameter of the pelvis was under three inches. This instrument may possibly have been suggested by the Almsdachs, described by Albucasis, and figured by Churchill, but at all events it has come to us through Baudelocque and the French obstetricians, and has now, after successive modifications at home and abroad, been brought to such perfection that by its aid women whose pelvises measured but little more than one inch, instead of the three of Assalini, in the conjugate diameter, have been safely delivered of full grown children.

From Italy, again, we have learned another method of drawing the head through a very narrow pelvis. When the cephalotribe is used it flattens the roof of the skull on the base, and, without breaking this latter, turns or cants it so as to make it come down edgeways; but the brothers Lollini conceived the idea of breaking up the base so as to allow the bones composing it to glide and fold up on one another, and so come through a very narrow space. This they did with a *forceps tarière*, or augur forceps, with which they drilled holes in the base of the skull and so broke it up. This method has been improved on by Dr. Hubert, of Louvain, who, two years ago, read a paper before the Royal Academy of Medicine of Belgium, on an operation he called *sphenotresie* or *transformation*, which consisted in driving a large wedge-shaped screw into the body of the sphenoid, so as to split up the base into a number of pieces; and, about the same time, Sir James Simpson exhibited, at a meeting of the British Medical Association, an instrument, very similar in principle,

to effect the same object. From Belgium we have had another plan for reducing the size of the fetal head in the *forceps scie*, or saw forceps of Van Heuvel, of Brussels, with which the head is seized and then cut into segments by a chain-saw; and, more recently, Dr. Barnes, of London, has proposed to effect the same division of the head into segments by means of an *écraseur*. We have thus got at our disposal various methods of reducing the head or other parts of a child in utero to such dimensions that it may be drawn through a very narrow pelvis; and I claim it as another example of progress, that by some one or other of these methods in those cases where the pelvis is so deformed, that some years ago we would all have admitted that delivery could only be effected by that most fatal operation, the Cæsarian section, the child can now be removed with comparative safety to the mother.

BI-POLAR VERSION.

In another class of labours, mechanical skill has done much to overcome difficulties that but too often compromised the life of both mother and child. When the child lies with its long axis crossways, it must be turned before it can be born, or be doubled up, or extracted piecemeal. There are few operations in midwifery attended with more danger than version; the introduction of the hand into the uterus is always a dangerous proceeding; and various attempts have been made to effect the version without it, but it remained for Dr. Braxton Hicks to show that by a combined external and internal manipulation, that is by a hand placed externally on the abdomen of the mother, and one or two fingers in the os uteri, the operation, which so frequently failed before, can, in a large proportion of cases, be effected with ease and certainty, and the child be safely tilted into the proper position.

DECAPITATION.

But in some cases of transverse presentation it is found impossible to safely effect the version in any way, and "this impossibility of turning the child had," to use the words of Denman, "to the apprehension of writers and practitioners, left the woman without any hope of relief." Some proposed to decapitate the child under these circumstances, but owing probably to badly devised methods of doing it the operation was seldom if ever practised, and another was adopted instead. This operation, evisceration, was seldom accomplished, according to Dr. Collins, under two hours; in one case he spent two hours and a-half at it, and I have seen others and have myself been as long at it. It is to the operator two hours of hard work and great fatigue, and if it produces great exhaustion in him what must it be to the mother. After a series of attempts the operation of decapitation has lately been rendered so simple and easy of performance that it will probably altogether supersede

evisceration. Prof. Pajot, of Paris, has devised a method by which the operation may be effected in a minute and a-half; and I have myself done it with his instrument in this time. It is probable his method may be still further simplified, and in the paper read at one of our meetings during the past session I have myself described a modification of it that will, I hope, make it more easy.

HÆMORRHAGE—TRANSFUSION.

After the birth of the child, and especially when the uterus is fatigued and exhausted by prolonged labour, hæmorrhage is of not infrequent occurrence, and sometimes it is of a formidable character. We are indebted to Dr. Barnes for the introduction of a method that seldom fails to control this. I mean the injection of a strong solution of the perchloride of iron into the interior of the uterus. In the session before the last we had several successful cases of this mode of treating post partum hæmorrhage, brought before us by Dr. Roe, and during the same session Dr. Beatty brought before us a case of extreme hæmorrhage, in which the operation of transfusion was successfully performed by Dr. Robert M'Donnell, and I believe we shall have before us at our next meeting two other cases of transfusion, in one of which the same successful result was obtained, but in the other the operation was performed too late to be of any use. If further experience confirm the teaching of these successful cases, that the operation can be performed without producing phlebitis or other injurious results, it will, no doubt, be had recourse to at a much earlier period than hitherto, and we shall have no more failures from being too late. To inject blood into the veins of a patient dying from hæmorrhage is a natural impulse, and has been attempted from the earliest periods. The success of Dr. M'Donnell's method depends on a happy application of physiological knowledge and mechanical skill. The difficulties to be overcome were twofold—the risk of obstructing the circulation by the introduction either of a clot or of air. As soon as it was learned that the fibrin was an excrementitious material, and not the essential portion of the blood, it was at once seen that the risk of introducing clots could be obviated by the removal of all the fibrin, or clot-producing material; and this is the first step in Dr. M'Donnell's process. To prevent the introduction of air is the remaining difficulty. This is overcome by causing the blood to flow in by its own gravity rather than by a syringe, and by making it pass, in part of its course, through a narrow glass tube, where a bubble of air, if it occur, can easily be seen, and its passage be at once arrested.

LACERATION OF THE PERINÆUM.

Laceration of the perinæum during labour, especially in primiparous labours, is very common, and occurs, as has been shown by Sir James

Simpson, "despite every modification of management, and in cases also in which no kind of management has been adopted." This also has been charged against the forceps, and, at one time, it was taught that the forceps should never be used in a first labour, because of the danger of lacerating the perinæum. Most obstetricians of the present day, however, will, I have no doubt, agree with me in saying the progress of the head is more under control when the forceps is used than in the natural labour of strong and impetuous young women, and that the perinæum is actually safer when the forceps is skilfully used, than when no assistance is given. When the accident does occur, if the laceration do not extend too close to the sphincter ani, the immediate consequences are so trivial that, till recently, it was not thought necessary to adopt any special treatment to remedy the evil; but as the women who have had the perinæum torn, advance in life, they often suffer from prolapse of the posterior wall of the vagina, dragging down the rectum with it, a form of prolapse most difficult to relieve. It has consequently become the rule with many, when lacerations occur, to draw together the edges of the wound with one or two sutures, and, though the wound is a lacerated one, perfect union almost always takes place, and thus women are saved from much discomfort, so, though apparently a trivial matter, I think this an improvement not unworthy of a passing notice.

WASHING OUT THE UTERUS.

I have thus followed labour from its beginning to its conclusion, indicating some of the improvements of recent times in its management. There are many other points to which I would wish to allude did time permit, among others the induction of premature labour, and the management of placenta prævia by dilating the os uteri with caoutchouc bags, and some others of less importance, but it would weary your patience. I shall therefore conclude with a reference to a suggestion of Braxton Hicks that I think of too much importance to be passed over.

It sometimes happens in the course of convalescence after labour or abortion, that the discharges from the uterus become putrid and exceedingly foetid, from the presence of some decomposing substance within the uterus itself. This sets up fever and blood poisoning, which often run on to a fatal termination; but Dr. Hicks has shown, that with certain precautions the cavity of the uterus may be safely washed out, and the offending substance removed. It was at one time thought exceedingly dangerous to inject fluid of any kind into the cavity of the uterus; but it is now established, that if the os be sufficiently open to allow the fluid to run out again freely, there is neither danger nor pain in the operation; and I am quite sure that I have seen more than one woman relieved of a very serious train of symptoms by having her uterus thoroughly washed

out with a solution of permanganate of potash, as recommended by Dr. Hicks.

Gentlemen, I shall not trespass on your patience any further, and I now conclude with thanking the Members of the Society for the honour they have done me in placing me, during the past year, in the Presidential Chair.

SECOND MEETING, 16TH DECEMBER, 1871.

Transfusion in Post-Partum Hæmorrhage. By A. HILL RINGLAND, A.B., T.C.D.; Licentiate of the King and Queen's College of Physicians; Assistant to the Masters of the Coombe Lying-in Hospital; Demonstrator of Anatomy in the Ledwich School of Medicine, &c. &c.

To the historian and physician the 17th century is alike interesting; the former cannot forget the great chain of events following and consequent on the accession of Charles I. to the English throne, the latter remembers that then flourished the great John Hunter, a man of whom it has been truly said, "A greater never lived." The historian contemplates with pride the rapid advancement of civilization so well commenced and carried on at that period. The physician meditates with equal satisfaction on the discovery of the circulation of the blood, and on the invention and employment of the operation of transfusion, two circumstances which would alone tend to render any age famous.

The introduction of the operation of transfusion into the practice of medicine at about the year 1667, was the starting point of one of the most violent controversies that has ever agitated the world of medical science; a controversy which in time (especially on the continent) degenerated into a contest of the most virulent scurrility, in which both opponent and advocate of the operation so far forgot their self-respect and professional dignity as to denounce one another in most abusive terms.

The invention of this operation, and the bringing it into prominent notice, is, undoubtedly, due to those who lived at this period, yet the general idea of transfusion may not have been completely absent from the minds of those mighty masters in ages long previous, "who," to quote the words of a happy writer, "first discovering the principles of things, have left us who follow them on the face of this our planet, only the less splendid honour of exploring those tracts of knowledge which they originally pointed out."

Some physiologists, in support of this idea, contend that the operation of transfusing blood and other fluids into the human system is one of extremely ancient date; and they ground their opinion on some obscure allusions to be found in the writings of a celebrated ancient poet. Ovid, in the seventh book of the *Metamorphosis*, represents Medea as

renewing the youth of the aged Æson, by injecting into his veins the sap of some peculiar herb—

——— “*Veteremque exire cruorem
Passa replete succis;*”

And again, some fifty lines further on, the same clever poet graphically describes the fiend-like imposition practised by Medea on the daughters of Pelias, in the passage ending with the phrase,

“*At repleam vacuas juvenili sanguine venas.*”

Yet the premises in both these cases can be scarcely considered as sufficiently defined to warrant such a definite conclusion. The probability is that, as Ramsbotham remarks, “the expressions relied on are nothing more than a poetic method of describing the intention of Medea to restore Æson to youth;” as, indeed, the whole context abundantly shows.

It is a matter of doubtful speculation whether France or England has a right to claim the honour of this great invention; for, at about the same period, Lower and King in the latter country, and Davys and Emmerets in the former, advocated its employment in many diseased conditions, and demonstrated its usefulness and practicability by numerous experiments on the lower animals. The blood of these latter, and not human blood, was the kind used by these experimenters; a dog, calf, sheep, or rabbit being the animals generally victimized.

Great and manifold were the difficulties which interfered with the advancement of the operation: ignorance and prejudice on the one hand, and mistaken zeal on the other, caused the contest before alluded to, to become the general topic of the day—the eager and pregnant subject of debate in every circle of society, from the scientific physiologist and practical physician to the ignorant cottager and empty-headed courtier; nor was it until some thirty years after the discovery of Harvey that the public mind would tolerate even the mention of transfusion. Then the cause of all the “ills which flesh is heir to” being ascribed to some peculiar condition of the circulating fluid, the tide of public opinion suddenly changed, and swept away in its headlong course all preconceived prejudices; it was then thought that the means of curing all diseases had been at length discovered, and that, in order to alleviate bodily anguish and mental anxiety, in order to restore to the full vigour of health and strength the victim of a contagious fever or dangerous pestilence, it was only necessary to remove the diseased blood, and replace it by pure, drawn from a sound and healthy animal.

Remembering the primitive and comparatively unscientific manner in which the operation in its infancy was performed, bearing in mind the quality of the blood injected, and also its disproportionate quantity;

remembering still further the frequency of its employment, it surely is not surprising to find as a consequence an inordinately high rate of mortality. Many sad events happened which considerably tended to abate the prevailing enthusiasm; a few important individuals (among whom may be included a prince of the blood royal) having died from the effects of transfusion, the authorities of Paris prohibited, under heavy penalties, the use of the operation; and the head of the Roman Catholic Church threatened with excommunication those who should employ such a method of procedure.

From this period until the time of Blundell, a century and a half later, transfusion was seldom or never employed, and, in fact, the operation was generally regarded as one utterly useless, and even extremely dangerous.

Dr. Blundell meeting with many cases of severe uterine hæmorrhage in his extensive practice, and seeing many patients succumb to this terrible complication, deemed it advisable to re-introduce this once popular remedy; and much of the success which transfusion has attained in the present day is no doubt due to the inventive genius and practice, as well as theoretical ability displayed by that great obstetrician. Instead of the simple tubule formerly used, a portable syringe was invented; instead of the blood of brutes, which, for obvious reasons, was not always to be obtained, the use of the human vital fluid was strongly advocated; instead of the animal from which the blood was taken being in direct connexion with the patient who was the subject of the operation, it was demonstrated, by repeated experiments, that "the sanguis could be received into a vessel and passed through a syringe without being thereby rendered unfit for the purposes of life."

Of all the changes and improvements made in the operation by Blundell, none seemed, in the opinion of his contemporaries, to militate more strongly against its future success than the small quantity of fluid recommended as necessary. "It seemed" (to use the words of Davis) "to be a violation of probability itself, that an adult female, previously in good health, and therefore containing many quarts of blood immediately before the time of her confinement, could possibly recover from a state of well nigh fatal hæmorrhage by the paltry addition of a few ounces of blood." That Blundell's theory was correct in this particular experience has abundantly proved.

After Blundell's time little was heard of the operation for a considerable number of years.

Many and valuable papers on transfusion have appeared within the last quarter of a century, and have considerably increased our physiological and scientific knowledge of the subject.

Dumas and Prevost, in 1821, discovered that the blood of one species being introduced into the system of another, a form of pyæmic blood-

poisoning was subsequently produced, but that the most satisfactory results followed the introduction of the blood of one animal into the circulatory system of a creature of the same species.

The results arrived at by Bischoff, published in 1833-38, are interesting and important. This writer gives it as his opinion that the red corpuscles are the principal life-restoring constituents of the blood; that they are in no degree altered by the continual stirring of the fluid; that this agitation causes the blood to be defibrinated; and that the danger of clots being introduced is thus considerably lessened. This writer corroborates the statement of Dumas and Prevost with regard to the danger of introducing the blood of one species into that of another, and recommends strongly the use of *venous* instead of arterial blood. Professor Martin of Berlin published, in 1859, a treatise in which he strongly advocates the use of transfusion as a curative agent for dangerous hæmorrhage occurring in the pregnant and parturient condition, and lays much stress on the necessity for the complete defibrinization of the blood. Dr. Graily Hewitt, in a communication to the *British Medical Journal*, August 29th, 1863, ably supports Martin's propositions and statements.

If time permitted I might quote extracts from the works of Routh, Soden, Blasius, Brown-Sequard, and Laudois, all of whom have written on the subject, but sufficient has been said to prove that transfusion, with all its details, has largely occupied for some years the attention of many distinguished men in different parts of the globe.

To render transfusion as safe an operation as possible, many instruments have been invented, and many modes of operating recommended; none of these, however (in the opinion of those capable of giving one) equal for mechanical completeness and extreme simplicity the recent invention of Dr. Robert M'Donnell, which lies on the table before you.

This apparatus (to borrow the words of the inventor himself) "has the advantage of dispensing with the use of the syringe, which was so ill-adapted to operations of emergency, as the piston has often ceased to be air-tight when it is necessary it should be completely so." This is an important change, for though the operation can be satisfactorily performed by using a common brass enema syringe, as was effected in the celebrated case which occurred in the practice of Dr. Beatty last year, and so graphically detailed by him to this Society, still any improvement or modification which lessens the dangers of transfusion increases in the ratio the chances of success.

In this apparatus, instead of the syringe, a strong glass pipette is used, the blood contained therein being forced into the vein by gravitation, or if that be insufficient, by the pressure caused by the blowing into the mouth of the instrument. The orifice of the pipette is funnel-shaped, and must be only of such a size as to be completely closed by the application of the thumb of the operator or assistant. The bulb

should be capable of containing six to eight ounces of fluid. The inferior portion of the pipette should be tapered off, so as to be easily inserted into the india-rubber tube which connects it with the silver nozzle. This latter should be probe-pointed, and at the base about the thickness of a No. 6 catheter; there must be no opening at the end, but a small one in its side, about an inch from the probe-point. This is undoubtedly another and vast improvement in this instrument, over every other apparatus previously in use, as it only necessitates the partial withdrawal of the nozzle beyond the lateral opening, should an air-bulb be discovered in the supplying tubule, thereby affording a free exit thereto, and permitting the immediate replacement of the nozzle within the vessel. Anyone who has witnessed the operation, and the complete contraction of the vein under the influence of the atmosphere, and also the immense difficulty and consequent delay of subsequently discovering it, will see at a glance how great an improvement this is. These details as to the formation of the instrument, I have taken in a great degree from a short communication on the subject of transfusion, published by Dr. M'Donnell last year, in the Nov. number of the *Dublin Quarterly Journal*.

It may not be amiss here to mention that in last year's January number of the Parisian Magazine, *Archives de Physiologie*, there appears an interesting article on the subject of transfusion by M. de Bellina. In this an ingenious instrument for the performance of this operation is accurately described, with an illustration. It consists of an oblong vessel, shaped somewhat like a spirit flask, at the superior portion of which is placed a small air-pump made of india-rubber. This, like our ordinary injection apparatus, is capable of being compressed by the hand. The pump is connected with the flask-like vessel by a small tube about (to use the French system of measuring) two centimetres long, in which there are placed two separate valves, in order to arrest the entrance of the organic germs suspended in the atmosphere into the fluid to be transfused. At the inferior portion of the vessel is attached a black india-rubber tube, with which is connected the "trocart d'infusion," constructed according to the author's direction. In making and using this instrument, the piston portion of a syringe is dispensed with.

The apparatus which I have thus briefly described, ingenious as it undoubtedly is, does not, to my mind, equal in simplicity that of Dr. M'Donnell.

It has only once been my privilege to be present during the performance of the operation of transfusion by Dr. M'Donnell, the particulars of which case I will now briefly lay before you.

At 2:30 o'clock a.m., on the morning of the 25th of September, 1871, I was requested to see a woman living in Tenter's-lane, off Weavers'-square. On my arrival I found that the labour, which was tolerably rapid, had been completed in about two hours. On making the few

necessary inquiries concerning the former history of the case, I was told that this was her tenth labour; that she was during her confinement always prone to hæmorrhage; that the last thrice the placenta had been manually removed, and that her life, about eighteen months before, was saved only by the timely injection of the tinct. ferri perchlor. into the uterine cavity by my friend and colleague, Dr. Roe, the then assistant to the masters of the Coombe Hospital. Mr. Wier, the resident apothecary of the hospital, who was then in charge of the case, further informed me that the pupil midwifery assistant, in consequence of dangerous hæmorrhage, had felt himself constrained, without waiting further assistance, to pass his hand *in uterum*, and remove the placenta, which he found morbidly adherent. The condition of the patient when first seen by me was as follows:—

She was lying on left side in the usual obstetric position, hips raised and head low; extremities cold; surface of body covered with a cold clammy sweat; respiration laboured; eyes almost perfectly insensible to light; pulsation in radial or carotid arteries scarcely to be felt, notwithstanding the constant administration of stimuli; conscious and able to speak, but voice almost inaudible; weak cardiac impulse; complete absence of the second sound of the heart; uterus fairly contracted; *hæmorrhage completely arrested*, Mr. Weir having maintained constant and steady pressure on the uterus from the time of his arrival, three-quarters of an hour previously. Seeing the serious nature of the case, I sent a messenger without a moment's delay for the senior master of the hospital, Dr. Ringland, and at the same time for Dr. M'Donnell, believing the operation of transfusion to be the only chance for the patient. Both of these gentlemen arrived at the same moment, having come to my assistance in the briefest possible space.

The condition of our patient now was by no means encouraging, for in spite of the almost incessant use of brandy, the application of sinapisms to the calves of the legs and cardiac region, the placing hot bricks and warm bottles to the extremities, all steadily employed from the moment of my arrival, and indeed before it by Mr. Wier, the following evidences of approaching dissolution were painfully manifest:—Complete unconsciousness, death-like pallor of face, falling of the lower jaw, complete absence of arterial pulsation at the wrist, and relaxation of the sphincter ani.

Though the chance of success was indeed small, we were all of opinion, *hæmorrhage now being entirely arrested*, that the operation of transfusion should be performed as the last and only possible resource. No sooner was the result of our consultation known to the pupils present, than several of them volunteered the one thing required—the blood! Foremost among these, however, was Mr. Anthony Cassidy, whose offer was at once accepted. Dr. M'Donnell, in his interesting communication before alluded to, and Dr. W. H. O'Leary, in an introductory address

to his class attending St. Vincent's Hospital this session, have paid a just and well-deserved tribute to the generous self-denial and true heroism always displayed when occasion requires by the students of the Dublin schools of medicine; and the above-mentioned fact fully corroborates their high opinion. The mode of operation in this case was as follows:—

The operation of venesection being performed on Mr. Cassidy in the ordinary manner, the blood was allowed to flow into a small bowl, which previously had been heated with hot water. All the time it flowed it was stirred by a long glass rod in a uniform direction. At the end of a very few minutes the process of defibrinization was completed by straining the fluid through a very fine piece of muslin into a small vessel floating in a basin containing a quantity of water, at a temperature some few degrees higher than blood heat. The pipette was then filled with blood by the force of suction applied at the orifice, after which a small spring forceps was applied to the india-rubber tubing a short distance from its lower extremity. The pipette, filled and ready for use, was then placed in a small jug filled with water at about 104° Fahr., and kept in it until a vein had been opened in the patient's right arm; immediately previous to which an acupressure needle was passed under the vessel to isolate it from the adjoining tissues. The probe point of the nozzle was next carefully introduced a short distance into the open vein, the spring forceps removed, and the blood allowed to descend until it appeared at opening in nozzle, by which means all air was excluded; and finally the nozzle was passed in a considerable distance, until the lateral opening was completely within the canal of the vessel. Though the pipette was raised a considerable height, the weight of the column was insufficient to propel the blood onwards, considerable pressure from the mouth of the operator applied to the bulb being necessary. While the blood flowed all eyes were watching the pipette, as well as the small glass tube near the termination of the india-rubber tubing, in order to warn Dr. M'Donnell when to cease applying the propulsive power needed. About 8 oz. was transfused, but almost before the operation had concluded a deep convulsive gasping sob was heard, followed by complete cessation of the cardiac and pulmonary actions, and our patient was gone beyond the reach of human aid; she was dead.

In the performance of this operation two difficulties are generally encountered, and must needs be overcome—1st, the finding a vein in your bloodless patient; and, 2nd, the being perfectly sure the silver nozzle is properly in the vein when found. In the case now detailed the amount of force taken to propel the blood was exceptionally great, and complicated it still further. The pipette being made of glass, and the tube connecting the pipette with the nozzle being composed of india-rubber, an accident may possibly occur to either of them during the

periods of preparation for, or in the course of this operation, which might seriously mar or altogether preclude its performance. The water in which the pipette is placed may be of such a temperature as to cause sudden fracture of this portion of the apparatus, or a similar casualty may result from many other causes; and the force employed to propel the blood into the veins may be so great as to cause rupture of the tubule, which, indeed, may become hard and liable to crack by mere age.

To meet either of these contingencies it may be expedient to have in the case two pipettes, and several tubes renewed from time to time.

Experience has now taught us that the operation of transfusion is desirable; nay more, is the last and only resource available in many cases of uterine hæmorrhage; still we should, on the one hand, ever be on our guard not hastily to have recourse to this operation, lest a practice of such moment, of such vital importance, be exposed to unmerited reproach by employing it in cases in which a favourable termination without its aid is still within the limits of hope; whilst, on the other, the particulars of the case which I have now related prove beyond all question that "*transfusio sanguinis*" should not be deferred or delayed until sensibility is extinguished, arterial pulsation obliterated, cardiac action barely perceptible, respiration enfeebled, and the powers of subsequent reaction completely paralysed.—*December 16th, 1871.*

A Case of Post-Partum Uterine Hæmorrhage successfully treated by Transfusion. By JOHN RINGLAND, A.B., M.B., M.D. Univ. Dub.; M.R.I.A.; Fellow and Censor of the King and Queen's College of Physicians; Ex-President of the Dublin Obstetrical Society; Senior Master of the Coombe Lying-in Hospital; Professor of Midwifery in the Ledwich School of Medicine; formerly Vice-President of the Association of the Members of the College of Physicians, &c., &c., &c.

The deep interest manifested in the subject of transfusion as the ultimate treatment for uterine hæmorrhage when all else has failed, and the wrapt attention paid to the details of the very interesting case submitted to this Society by Dr. Beatty, on April 9, 1870, induce me to believe that the history of an analogous case would not be unacceptable. The communication^a just now made to the Society enters so fully into the history of transfusion in hæmorrhage, the various steps and difficulties of the operation, and the instruments employed for its successful performance, that it is altogether unnecessary for me to do more than briefly to submit the particulars of the case to which I wish to direct your attention.

The lady, the subject of this memoir, was delivered of her third child

^a Vide the present number of the Dublin Journal of Medical Science, page 78.

at half-past six o'clock on the evening of Tuesday, 3rd October last, after a very easy labour, in the course of which nothing abnormal presented itself. She was in her twenty-third year; her first child had been born abroad, and her second in London, and on both occasions violent flooding attended or followed the labour; her life had, moreover, been on two occasions placed in imminent jeopardy by hæmorrhage connected with miscarriage. On the present occasion the secundines were expelled, perfect and untorn, in less than ten minutes after the birth of the child, by the natural efforts unaided in any way, and the discharge was in no wise in excess; but, in consequence of the hæmorrhagic diathesis manifested by her history, I administered two doses of the saturated solution of secale cornutum, with an interval of twenty minutes, and retained a grasp of the uterus for an unusually long time after the expulsion of the placenta. I, moreover, for a considerable period after the application of the binder and pads, maintained pressure over that organ, and did not leave her bedside until eight o'clock, when the nurse-tender took charge. On my return to the room, after the lapse of less than half an hour, I found that hæmorrhage had set in, the blood pouring from her in a profuse stream. The uterus was not very enlarged, but was soft and doughy; her countenance was bright and cheerful, but her pulse, rapid and compressible, indicated already that the quantity lost was not merely considerable, but had told damagingly on her system. The uterus responded well to the grasp of the cold hand, but almost immediately relaxed, and the hæmorrhage returned with renewed violence. Brandy and ergot were administered, with the effect respectively of rousing the circulation for a time, and of inducing uterine contractions, which were, however, but temporary. Flipping the vulva and nates with napkins wrung out of cold water, the injection of cold water into the uterus, and the introduction of the hand into its cavity, were in succession had recourse to, but with no better result, contraction following the employment of each respectively, but relaxation instantly succeeding. Owing to the accidental breaking of my bottle of perchloride of iron, I was for a time unprovided with any of that styptic; but convinced of the urgency for its use, I sent a messenger for a supply. Meanwhile Dr. George Johnston, whose aid I had summoned, arrived, and relieved me in my efforts to grasp the uterus, when he, too, experienced the alternate contraction and relaxation of that organ. With his sanction I now injected into the cavity of the uterus—taking care that the orifice of the tube of the instrument was well up to the fundus, and that there was ample space at the os to permit the free return of the fluid—about half a pint of a solution of the perchloride of iron, containing one part of the saturated solution in glycerine, and three parts of cold water. Subsequent to its employment not one drop of blood was lost; we were consequently now enabled to pad and bandage her, warm jars having been applied to

her feet, legs, and thighs, and she herself made as comfortable as possible under the circumstances.

While the various steps already detailed were in progress, she was, from time to time, freely supplied with cold beef-tea, brandy and ammonia, and smelling salts were applied to her nostrils, but still the pulse indicated excessive prostration of the circulation, varying from 140 to 180, and being intensely compressible, the least pressure, even when it was most favourable, almost totally obliterating it, while at times it was not at all distinguishable. She, moreover, had become very restless, was tossing about in the bed, sighed frequently, and her respiration became very laboured.

It now became manifest to both Dr. Johnston and myself that matters were assuming a most alarming aspect, and that a fatal issue was all but impending; and, on my suggesting to him in a low voice, so as not to be audible to our patient, the advisability of being prepared for transfusion, he entirely concurred. Accordingly I sent a messenger requesting the immediate attendance of Dr. Robert M'Donnell, who, it will be recollected, performed the operation in Dr. Beatty's case as well as in that reported in the communication just now read to the Society, and who, by his many experiments, and by the invention of the perfect but simple instrument he now employs, has made the operation, in Ireland at least, almost his own.

While awaiting his arrival, we turned our attention to the means of procuring the necessary quantity of blood, in order that no time should be lost after he reached us. Considerable difficulty, however, in this respect, presented itself; our patient's husband was abroad, her brother-in-law—in whose house she was a guest—was absent in the country; her sister had been delivered but a few days previously; and we would not accept her willing offer to supply the required blood; nor would we abstract any from her brother, whose delicacy from infancy rendered such a proceeding altogether inexpedient; whilst nothing could induce any member of the household to volunteer the supply of what was so much needed. In our difficulty I suggested to Dr. Johnston the possibility of one of the students attending the practice of the Rotunda Hospital, volunteering, knowing well, from a long experience of them, that whenever the advance of science, or the call of humanity demanded a personal sacrifice, the medical students of Dublin, whatever else, whether truly or falsely, may be alleged regarding them, have ever been, and, I am sure, ever will be, among the foremost to forget self, and make any sacrifice when important interests are at stake.

In the present instance our anticipations were not disappointed, Dr. Johnston, returning in a marvellously short time, accompanied by the volunteer, Mr. Andrew Irwin, of Sligo, a Licentiate of the College of Surgeons, and at that time resident pupil of that institution, whose name

I take this opportunity of recording, and whom I now thank in the name of Science, the advance of which he thus aided; of our noble profession, of which, I trust, he will become an ornament; of humanity, the cause of which he has materially served; and of myself, on whom he has thus conferred a lasting favour.

It was eleven o'clock before we had all assembled, and we then found that a slight improvement in the circulation had taken place under the influence of the stimulants which had been steadily pressed; we accordingly determined on postponing the operation for some time longer, in the hope that reaction might be still further established; but in this we were disappointed, for, although the pulse occasionally returned to the wrist, it did not continue perceptible for any lengthened period, and the intervals of its absence became more prolonged; until eventually, at 12 o'clock, it was altogether wanting at the dorsum of the foot, at the wrist, and along the whole track of the artery up the arm; whilst the second sound of the heart was entirely absent, and the first was very feeble.

It now became painfully manifest to us all, that no human aid could avail her, save by transfusion; it is right, however, here to mention, that she presented several good points which afforded reasonable ground for hope for a successful issue to the operation; the temperature of her body was but little below the natural standard; her lips, to a considerable extent, retained their colour; her voice remained unaltered in volume, and her faculties were clear and unimpaired. She perfectly understood the cause and purport of the contemplated operation, and when her sanction was sought for, promptly assented to its performance, which was accordingly at once proceeded with by Dr. M'Donnell, who used his own beautiful instrument—beautiful, because of its extreme simplicity—a description of which is to be found in the *Dublin Quarterly Journal*, Vol. L., p. 263.

About sixteen ounces of blood, drawn from Mr. Irwin's arm, afforded about fourteen ounces of defibrinated blood; sufficient, in fact, twice to fill the pipette, which is made to contain seven ounces. The skin over and in the course of the median cephalic vein in our patient's right arm having been pinched up, an incision was made through it in a direction at right angles with the vessel, which, on the separation of the lips of the wound, was at once exposed to view, and beneath which an acupuncture needle was at once passed to preclude its being lost to sight in the empty and contracted condition it would speedily of necessity attain to. The vein was, when first seen, of greatly diminished size, but ejected a small stream of blood immediately on being opened, and this added another favourable feature to those already mentioned. The nozzle of the instrument passed into the vein with, to all appearance, considerable facility, but gravitation produced no perceptible diminution in the

quantity of blood in the pipette; nor was pressure by the mouth applied to its open extremity, and employed with no inconsiderable force, followed by a much better result. The nozzle was now withdrawn, but the vein having contracted considerably, as was anticipated, owing to its exposure to the cold atmosphere, great difficulty was experienced in finding the incision made into it and of introducing the nozzle within it; and the importance of having passed the needle beneath it at the commencement of the operation now became manifest, as, even with this precaution, it was not until after nearly half an hour of extreme anxiety—during which the coolness, presence of mind, and surgical skill of the operator were tested to the utmost, and were not found wanting—that its introduction was at length effected. Still gravitation availed not, and great pressure with the mouth had to be resorted to, to force on the fluid. During all this time there was not any pulsation whatever to be felt at the wrist, although frequently and anxiously sought for; but scarcely had more than three or four ounces of blood been transfused than, to our great joy, a pulse—scarcely perceptible, 'tis true, but still undoubted—was found. The blood was still passed onwards, until the whole supply at our command was exhausted, and by this time the circulation was not merely distinguishable, but perfectly manifest. The quantity introduced, allowing amply for all possible waste, must have been not less than twelve ounces.

An anodyne draught, containing forty minims of tincture of opium, was now administered, and repeated after half an hour; and cold beef-tea and brandy were alternated every hour. Perfect quietness speedily ensued, her respiration became more tranquil, and after two hours she fell into a quiet but brief sleep; on awaking from which her pulse was 140, but, as might have been expected, very feeble. She was now placed on quinine and opium, in doses of one grain of each, to be repeated every hour; and this treatment was steadily persevered in, without intermission except during sleep, until she had taken 86 of these pills, when the interval was lengthened to two hours and subsequently to three. Altogether she took 109 grains of each with the most complete tolerance, and with no other result than her steady progress to convalescence. On the third day after the operation there was slight hardness and pain on pressure of the vein to the extent of about an inch and a half upwards from the incision, which, however, yielded to a single application of the 30 grain solution of nitrate of silver, and afforded no further cause for anxiety. A simple *lavement* daily was sufficient to maintain regularity in the alvine discharges. On the tenth day—the opium and quinine pills having been omitted some time previously—night sweats supervened, which, however, yielded to the steady use of quinine, dilute sulphuric acid, and acid infusion of roses.

This lady is now, I am very happy to be able to say, perfectly restored

to health and strength, and able to enter into, and enjoy the ordinary avocations and pleasures of life. In my history of her case, I may have appeared to some to have been more prolix than necessary; but I have been anxious to prove, by the very minuteness of my details, that transfusion in her case was an imperative necessity, and I conceive that I have fully demonstrated that our patient was snatched from death to life, and that her preservation was owing, under God, to the performance of that operation, and to it alone, when all other means had signally failed.—*December 16th, 1871.*

The PRESIDENT said,—No subject could be more important than that brought before the Society in the present communication. He had not himself witnessed any case of transfusion, but he had been on many occasions prepared to have it done, and had watched the cases carefully with Dr. M'Donnell's assistance, in order to perform the operation if necessary, and they had always, except in one case, postponed the proceeding. It is important to watch the right time to perform the operation, and he must say the case recorded by Dr. A. H. Ringland shows the importance of not delaying it too long. They have had two successful cases of transfusion, one recorded by Dr. Beatty, and one by Dr. Ringland, conveying this important lesson, enforced by the death in Dr. A. H. Ringland's case, namely, not to be too much afraid of the operation, and to have recourse to it before the vital powers of the patient are entirely exhausted. The first case in which he contemplated transfusion was a very sad one. It was that of a lady in the suburbs of Dublin, who had a miscarriage. When he saw her she had been bleeding from ten in the morning until four in the afternoon. He at once succeeded in removing the placenta and membranes, and there was no further bleeding, but the progress of the case was downward; she gradually became lower and lower. He sought the assistance of Dr. Beatty, and, when he arrived, at his suggestion, Dr. Robert M'Donnell, and the means of performing transfusion, were sent for. Everything was prepared, the husband afforded the blood, it was strained, and everything was ready, but, as they were about to open a vein in the arm, she breathed her last, and the case made a deep impression on his mind. The two successful cases now recorded by Dr. Beatty and Dr. Ringland should make us resort to the operation at an earlier period.

A remarkable paper appears in the *Lancet* of this day, giving an account of three cases of transfusion, two successful and one unsuccessful. There was this extraordinary feature about the unsuccessful case, that the defibrinated blood used had been drawn out the night before, and preserved for fourteen hours before being injected into the veins, but that does not appear to have had any influence on the result of the case. The operation was resorted to in that instance because of the exhaustion produced by a

gastric ulcer. The other two cases were cases of poisoning, one by phosphorus and the other by carbolic acid, and in these cases a quantity of blood was withdrawn equal to the quantity injected. The cases are very interesting, but they are recorded in such a manner that perhaps implicit reliance could not be placed on them.

Dr. ROBERT M'DONNELL said he might be allowed to thank the President, Dr. Beatty, and the two gentlemen who had read the papers that evening, for the complimentary terms in which they had spoken of him and his instrument for transfusion. Dr. Ringland had said he (Dr. M'Donnell) had made the operation peculiarly his own, but he had anything but a desire to monopolize it. He thoroughly believed in it, and it was his earnest wish that not only obstetricians but operating surgeons should make themselves acquainted with an operation which was easy in its performance with some practice, and which any ordinarily dexterous surgeon could make himself perfectly familiar with by practice on the dead subject. There were some operations, such as those for the removal of tumours and for aneurisms, &c., which could not be practised on the dead subject. There were others, of which transfusion was one, which could be thus practised; and that being so, it was inexcusable for any young surgeon not to avail himself of his opportunities to make himself familiar with the details of an operation which he might afterwards find necessary for the preservation of life. This was essentially an operation of detail. In the first instance, perfect cleanliness was indispensable. All the instruments, the tubing, the pipette, should be perfectly clean; the bowl in which the blood was received should be scalded with hot water, and the muslin used in straining the blood should be scalded also. In the second place, the only really difficult part of the operation when it came to be performed was the opening of the vein in the person into whom the blood was to be transfused. It was like taking up a vein in the dead body. There was this only difference between the dead and the living bodies—that in the latter, owing to the cold air getting at the vein, its muscular fibres contract, and it becomes very small. Hence the importance of passing a needle under it, so that one might always be able to find it when necessary. He believed that in both the cases that were detailed that evening the apparent difficulty found in putting in the blood arose from their natural anxiety to see it flow in quickly. You think the blood is not going in, when in reality it is going in, but very slowly. He had never found any instance, when practising on the dead subject, in which the weight of the blood was not of itself sufficient to make the blood run in without difficulty. The nozzle which had been described by him in the *Dublin Quarterly Journal* was an important part of the instrument.

It is of great importance to have a canula, with a probe point, and the

eyelet hole at the side, not at the end. The probe point was best suited for entering a small opening. They could easily make sure that all air was expelled before they put it in, and if during the progress of the operation a bubble of air was observed, it was possible to withdraw it, and allow the air to escape without any serious interruption of the operation. There was another advantage in an apparatus of the kind exhibited, well illustrated in the case detailed by Dr. Ringland. There was some delay experienced in getting at the vein in the woman's arm when all was ready for making the injection. In such a case it was only necessary to place the nozzle in the top of the tube, and the pipette could then be placed in a can of hot water, and allowed to remain there as long as the operator wished. It was evident that this was a great advantage, inasmuch as everything could be got ready for use without undue haste, and the operation could be carefully and deliberately performed without fear of the blood getting cold. He believed this operation was capable of being applied to a large number of cases besides those with which the members of that Society were so familiar in obstetric practice. He hoped it might be applied both in surgical and medical cases, not only in cases where accidents or ulcers had caused copious hæmorrhage, but in cases of the nature of chlorosis or of cholera. Where an injection of saline solutions into the veins might be considered advisable, the appliances, for the purpose of transfusion being now so much simplified, should encourage surgeons to lend their aid to physicians in cases of that kind.

He would mention an instance of a surgical case in which transfusion might have been applied with advantage. A man whose limb was torn off had come a long way from the country, and arrived at hospital exhausted from loss of blood; nevertheless it was deemed advisable to amputate, and he died within a day or two after the operation, but seemed to be run down by that kind of secondary fever which follows exhaustion from extensive hæmorrhage. Now he should think in such a case of saving the patient's own blood, unavoidably lost at the operation, and having it defibrinized, and then after the operation giving him back his own blood, thus giving him promptly what is equivalent to a good meal of nutriment to start him on his way to recovery. He believed the most important thing connected with this operation was establishing the desirability of introducing defibrinated blood. In former times it was held that fibrin was an important nutritive part of the blood; it was considered that the blood was deprived of its most essential element when the fibrin was taken away; but physiologists had now come to the conclusion that fibrin, instead of being an all-important nutritive element, was rather an excrementitious matter which was given to the blood as it returned from the tissues after doing its work, and that it was an advantage to get rid of it. His (Dr. M'Donnell's) physiological experiments

had proved this to be the case. He observed this when repeating the experiments of Dr. Brown-Sequard on limbs of animals removed from the body, on animals deprived of the head, and on the head removed from the body, whereby the vital phenomena were reproduced by keeping up an artificial circulation. He placed on a tray the head of a dog which had been severed from the body, and with a double action syringe injected the oxygenated and defibrinated blood of a sheep into the veins, producing artificial circulation through the brain of the decapitated dog. The vital phenomena were produced—the jaw opened, the tongue was protruded, the eye moved, but the blood which flowed out through the veins coagulated. Defibrinated blood was thrown in, but there was carried out, taken away from the tissues, fibrin that coagulated, thus proving that the fibrin found in the blood is an excrementitious matter carried away from the tissues. This remarkable fact, coupled with what they knew of the liver and kidneys, which were blood-purifying organs, namely, that the blood in the hepatic vessels and in the emulgent veins after leaving the kidneys did not coagulate, left little doubt that the fibrin was not a nutritive element of the blood. Hence, physiologically speaking, the defibrinated blood was better for the patient. He need not say it was important also in a surgical point of view. The risk of trying to inject blood not defibrinated was very great, and he was of opinion that the deaths which occurred several days after transfusion occurred from embolism. He said then that, physiologically, the defibrination of the blood made it better for the patient, and surgically it disarmed the operation of most of its dangers and its risks. With regard to the difficulty experienced in Dr. Ringland's case in getting the blood in, he was inclined to think that in all these cases it arose from their being rather precipitate, and thinking that the blood should go in faster than it really should. He had contrived a simple apparatus, by which a good deal more force could be put on, as in Bellina's apparatus by an indian-rubber bottle; but at the same time he did not recommend it. It was important that the blood should not flow in too quickly, and he believed that gravitation alone was sufficient to get it in—at least gravitation aided by the pressure of the mouth. Care should be taken not to have the blood too hot. The operator should be provided with a thermometer in every case, otherwise he ran the risk of producing coagulation of the albumen. The blood should be heated from 100° to 105°, at which there was no risk of coagulation, and this temperature could be easily maintained. There was one interesting point to which before sitting down he wished to allude. Some remarkable experiments, reported in the proceedings of the Royal Society, had been performed with the view of proving whether the breeding of rabbits could be altered by the transfusion of the blood of one kind into the veins of another. The experimenter transfused the blood of the lop-eared rabbits

into ordinary rabbits, and bred from the latter to try whether they would give birth to a lop-eared progeny. The result, however, was negative. The transfusion did not bring the spermatatic animalcules into harmony with the blood flowing through the veins of the animal. These experiments were brought forward in opposition to the theory of pangenesis of Darwin, and might at least serve to allay the fears patients had sometimes entertained lest they might partake of the peculiar character of the person whose blood was thrown into them. It was desirable of course that the blood should be taken from a healthy person, and, above all, they must take care that there was no syphilitic taint in the blood used.

DR. BEATTY had listened with great pleasure and instruction to the papers which had been read by Dr. Ringland and Dr. A. Ringland, and he congratulated the former on the successful issue of his case, and the latter on the diligence and skill with which he had brought forward all the accumulated knowledge on the subject, and the very lucid manner in which he had described the operation. It was rather a remarkable thing, and one on which they were justified in congratulating themselves, that this very rare operation should now have been three times performed in a short space of time, and twice successfully in this city. He wished to take that opportunity of returning thanks to Dr. Robert M'Donnell; for he had not the slightest doubt that it was to his perseverance and devotion to this particular operation these two successful cases were attributable. In the case which Dr. A. Ringland had described, they had a caution, not to delay the operation too long. In Dr. Ringland's case, he could not help it, because he was not called in time. He believed that many of those cases which they saw unfortunately die, might have been saved if this operation had been as well-known then as it was now, and he hoped it would become better known. There was a period in these cases of hæmorrhage after which they could not succeed, but there was a period up to which they might expect success if the operation were skilfully performed; and after the observations of Dr. M'Donnell, who told them that the operation could be practised on the dead body, it was the duty of every man who had hands and ordinary skill, to make himself familiar with it, so that in cases of urgency he might be prepared to save life with the very simple apparatus introduced by Dr. M'Donnell. In conclusion, Dr. Beatty stated that the patient on whom Dr. M'Donnell and himself had performed the operation of transfusion, was now in perfect health.

DR. JOHNSTON said that having seen fearful results from loss of blood after childbirth, he must corroborate Dr. Beatty's opinion that they should not wait too long to see the effects of the hæmorrhage, but at the proper time, when it was seen that all ordinary means had failed, they should interfere in this method, which Dr. R. M'Donnell had rendered so easy for

their adoption. Some short time since he was called to see a case of hæmorrhage in the fifth month. It was a case of abortion, and, he thought, owing to some oversight on the part of the physician, the whole of the membranes were not removed until a month afterwards, when severe hæmorrhage took place. Such was the state of exhaustion that when he (Dr. Johnston) arrived he found the woman perfectly pulseless. He found part of the placenta remaining, which he removed, and, by the injection of perchloride of iron of the usual strength, he succeeded in arresting the hæmorrhage, and then by stimulating the patient by enemata of beef-tea, wine, and opium, he brought back the pulse. If, however, he had failed with the remedies he applied, he would have sought the aid of Dr. McDonnell, and performed the operation of transfusion. Although he had been successful in the case referred to, he would not advise any gentleman to depend too much on perchloride of iron or any astringent mixture for arresting hæmorrhage.

DR. DENHAM must express the intense satisfaction he felt in listening to the papers of that evening, and the discussion which they had elicited. He looked at it as the most important meeting the Society had ever held. He had listened with great interest to the details of Dr. Ringland's case, and it passed through his mind at first, as it probably did through the minds of other members of the Society, that they had seen cases apparently quite as far gone recover. That idea, however, was largely dissipated when he came to hear the details and the result of the treatment after the operation. When he found the patient was able and required to take a grain of opium every hour for sixty or seventy hours, he concluded that the vital powers must have been at a low ebb. In that case the system was in the condition of a patient dying from cholera. Such a quantity of opium could not be introduced into a person in good health, and doubtless in this case a large portion of it was never absorbed, but passed down the alimentary canal without producing any effect on the system. The large quantity which had been administered, however, showed that the operation was called for. Every practitioner of midwifery, on looking back over his practice, must recall to his mind instances of valuable lives that might have been saved if they had been familiar with the operation of transfusion. He would mention one, a case which had made a great impression on him, of a lady who had bled to death simply from the application of six leeches. She was a person of delicate constitution, was confined safely, and went on well for some days. On the occasion of the milk fever there was some tenderness over the uterine region, and he ordered the leeches to be applied. The nurse ignorantly encouraged the bleeding by poultices. Another practitioner, not familiar with these cases, was called in, and he attempted unsuccessfully to stop the bleeding. He (Dr. Denham) was then sent for, after the bleeding had gone on for several hours. However he succeeded in commanding

the hæmorrhage from every vessel. It happened to be the morning of the funeral of his lamented and esteemed friend, Dr. Mayne, and, being anxious to attend it, he left a friend with the patient during his absence, and on his return from the funeral the patient was dead. The bleeding had been completely stopped, but the vital powers going down, the woman died. He had not the slightest doubt that her life would have been saved if they had been familiar at that time with the operation.

DR. CHURCHILL said all present must have listened with great interest and instruction to the points recorded in the papers themselves, and to Dr. M'Donnell's very instructive observations on the operation. He (Dr. Churchill) remembered Blundell's operation, and the operation fell through then from the accidents that occurred; and he had a strong suspicion that a good many of them might have resulted from the fibrin in the blood. The most important point was the defribination of the blood, for then there was no chance of embolism or other accident occurring. Blundell, he remembered, laid great stress on the blood being introduced very slowly. He set some one to watch the eyes of the patient, and the moment he saw a flicker of the eyelid he stopped the introduction of blood. If the blood went in by gravitation, as Dr. M'Donnell said, it could not go in too fast. As to the free use of opium after large hæmorrhage, he thought three-fourths of it had no medical effect. Dr. Haughton examined some time ago the body of a person who died of cholera, and who had got more than twenty pills with a grain of opium each, and he found all the pills in the stomach. He had heard of cases in which the effect of opium on the heart of the patient in cases of hæmorrhage was injurious, death having been caused in some instances. He thought, therefore, very great caution should be exercised in giving opium in cases of hæmorrhage, and he confessed he should be afraid of giving it to anything like the extent stated by Dr. Ringland.

DR. NUTTALL said the apparatus introduced by Dr. M'Donnell was so simple that any man with ordinary hands could use it, and it rendered an operation of great importance comparatively easy and safe. Some twenty years ago he tried the effect of injecting warm water, with salt and whiskey, into the veins of persons in collapse from cholera, and they were temporarily relieved; could sit up in bed and express themselves as being better than they were previously. Looking at this fact, it seemed to him that in cases where there might be a difficulty in getting blood, relief might be given in this way, with warm water, a little salt, and a small quantity of spirits. The object of using salt was, that it had a tendency to prevent coagulation of the blood.

DR. BYRNE also bore testimony to the great value of the instrument which Dr. M'Donnell had placed in the hands of the profession, and he had not the slightest doubt that when it came to be more generally known, it would be the means of saving many lives. These cases of

hæmorrhage after delivery were often most formidable and rapid in their effects; and he could easily conceive a case where the transfusion of eight, ten, or fifteen ounces of blood, would save the patient's life. Every one who had had experience in obstetrical practice must have felt the want of an instrument of this kind. They did not know the moment alarming hæmorrhage might arise; he himself had seen patients surrounded by every care and attention die from profuse bleeding, and he believed that were the instrument invented by Dr. M'Donnell known at that time, those lives might have been saved.

In reply to MR. F. T. PORTER,

DR. M'DONNELL said the only surgical case in which he had performed the operation of transfusion, was one of tetanus. It did not exercise any control over the complaint, but after the transfusion the patient was relieved from the sensations of hunger and thirst, which she had previously felt very acutely, proving that the sensation of hunger which was transferred to the stomach, arose from the want of nutriment to the system generally. The patient was continually saying, through her clenched teeth, that she was dying of hunger and thirst; but an hour after the operation she was relieved from these sensations.

DR. RINGLAND thanked the meeting for the manner in which his paper had been received. The subject all admitted to be one of great interest, and if his paper had produced no other benefit than eliciting the observations which had been made that evening, he thought he had done good to the Society. Dr. Denham said that it had struck him and others present that very probably the case might have recovered without any transfusion whatever, but that he was disabused of this notion when the subsequent details of the case were stated. He (Dr. Ringland) could only say in reply that it was the unanimous voice of Dr. Johnston, Dr. M'Donnell, and himself, that the time had come when nothing but the operation could save the patient. No matter how minutely the details of a case might be given, nothing, he thought, could come up to the observation of the persons who were looking on at it, and the conclusion they arrived at was that delay in performing the operation would imperil the patient's life. Dr. Churchill made an observation as to the toleration of opium in such cases. There was a great difference of opinion between Dr. Churchill and himself on that point. He thought that in hæmorrhage there was a greater tolerance of opium than in any other case. It was possible some of the opium might have remained solid in the canal; but after the vital powers were restored, when every hour for 86 hours one pill was given, he could not but think a solution of the pills must have taken place, showing the tolerance of opium by the system in such cases, and proving, as Dr. Denham said, that it was a case which imperatively demanded transfusion.

BOOKS RECEIVED, JANUARY, 1872.

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2. Notes on comparative anatomy; a syllabus of a course of lectures delivered at St. Thomas's Hospital. By William Miller Ord, M.B., Lond., M.R.C.P. London: J. and A. Churchill. 1871. Fcap. 8vo, pp. 203.

3. The science and practice of surgery. Illustrated by four hundred and seventy wood engravings. By Frederick James Gant, F.R.C.S., Surgeon to the Royal Free Hospital. London: J. and A. Churchill. 1871. 8vo, pp. 1265.

4. Symptoms and treatment of malignant diarrhoea, better known by the name of Asiatic cholera, as treated in the Royal Free Hospital during the years 1832, 1833, 1834, 1848, and 1854. By William Marsden, M.D. Fourth edition. Edited by Alexander Marsden, M.D. London: Wyman and Sons. 1871. Fcap. 8vo, pp. 13.

5. Mode d'invasion et de propagation du choléra, étudié à Smyrne. Par le Dr. W. Chasseand. Constantinople: Imprimerie M. de Castro. 1871. Fcap. 8vo, pp. 188.

6. Introductory lecture to the course of pathological anatomy at the University of Pennsylvania. By Joseph G. Richardson, M.D. Philadelphia: Lippincott and Co. Sewed.

7. On the treatment of hyperpyrexia as illustrated in acute articular rheumatism by means of the external application of cold. By Wilson Fox, M.D., F.R.C.P., Physician Extraordinary to Her Majesty the Queen. London and New York: MacMillan and Co. 1871. 8vo, pp. 78.

8. Fecundity, fertility, sterility and allied topics. By J. Matthews Duncan, A.M., M.D., F.R.C.P.E., F.R.S.E., Lecturer on Midwifery in the School of Medicine, Physician for Diseases of Women to the Royal Infirmary, Edinburgh, &c., &c. Second edition, revised and enlarged. Edinburgh: Adam and Charles Black. 1871. 8vo, pp. 498.

9. Anaesthesia, hospitalism, hermaphroditism, and a proposal to stamp out small-pox and other contagious diseases.

By Sir James Y. Simpson, Bart., M.D., D.C.L. Edited by Sir W. G. Simpson, Bart., B.A. Edinburgh: Adam and Charles Black. 1871. 8vo, pp. 562.

10. The Medical World. New York: William Baldwin and Co.

11. On intermittent malaise. By Dr. Henry Adams. London: H. K. Lewis. Sewed.

12. Digitalis: its mode of action and its use. The Hastings Prize Essay. By J. Milner Fothergill, M.D. London: H. K. Lewis. 1871. 8vo, pp. 89.

13. Mr. Spencer Wells's note book for cases of ovarian and other abdominal tumours. Third edition. London: J. and A. Churchill. 1871. Sewed.

14. A review of Darwin's theory of the origin and development of man. By James B. Hunter, M.D. New York: D. Appleton and Co. 1871. Sewed.

15. A handbook of therapeutics. By Sydney Ringer, M.D., Professor of Therapeutics in University College. Second edition. London: H. K. Lewis. 1871. Small 8vo, pp. 483.

16. The clinical thermometer: its lesions and teachings tentatively expressed in numbers. By Z. C. M'Elroy, M.D. New York: William Baldwin and Co. Sewed.

17. Sull' alcoolismo acuto, considerazioni del Cav. Dottor Ezio Castoldi. Milano: Stabilimento Dei Fratelli Rechiele. 1871. Sewed.

18. Neuralgia, and the diseases that resemble it. By Francis E. Anstie, M.D. (Lond.), Fellow of the Royal College of Physicians. London and New York: M'Millan and Co. 1871. 8vo, pp. 296.

19. Darwinism; being an examination of Mr. St. George Mivart's "Genesis of Species." By Chauncy Wright, Esq. London: John Murray. 1871. Sewed.

20. Transactions of the Pathological Society of London. Volume twenty-second. London: J. E. Adlard. 1871. 8vo, pp. 364.

21. Notes on the treatment of skin diseases. By Robert Liveing, A.M., M.D. Cantab., Senior Assistant Physician to the Middlesex Hospital. Second edition. London: Longmans, Green, and Co. 1871. Pp. 104.

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institutions, together with a proposal for organizing an institution for training midwives and midwifery nurses. By Florence Nightingale. London: Longmans, Green, & Co. 1871. 8vo, pp. 110.

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25. On the treatment of pulmonary consumption by hygiene, climate, and medicine, in its connexion with modern doctrines. By James Henry Bennett, M.D. Second edition. London: J. and A. Churchill. 1871. 8vo, pp. 190.

26. A practical treatise on Bright's diseases of the kidneys. By T. Grainger Stewart, M.D., F.R.S.E., Fellow of the Royal College of Physicians, Physician to the Royal Infirmary. Second edition. Edinburgh: Bell and Bradfute. 1871. 8vo, pp. 334.

27. Inaugural address, delivered at the opening of the twenty-second annual meeting of the American Medical Association. By Alfred Stillé, M.D. Philadelphia: Collins. 1871. Sewed.

28. On clinical education; the introductory address to the clinical session, 1871-72, at the Queen's Hospital. By Furneaux Jordan, F.R.C.S. London: J. and A. Churchill.

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30. Lecons de pathologie expérimentale. Par M. Claude Bernard. Paris: J. B. Baillièrre et Fils. 1872. 8vo, pp. 604.

31. De l'électrisation localisée et de son application à la pathologie et à la thérapeutique par courants induits et par courants galvaniques interrompus et continus. Par le Dr. D. B. Duchenne (de Bologne). Troisième édition, entièrement refondue. 1^{re} partie. Paris: J. B. Baillièrre et Fils. 1872. 8vo, pp. 576.

32. Traité des maladies des yeux. Par X. Galezowski. Seconde partie. Paris: J. B. Baillièrre et Fils. 1872. 8vo, pp. 888.

33. Etude medico-légale sur les blessures par imprudence, l'homicide et les coups involontaires. Par Ambroise Tardieu, Professeur de Médecine Légale: à la Faculté de Médecine de Paris. Paris: J. B. Baillièrre et Fils. 1871. 8vo, pp. 196.

34. Transactions of St. Andrew's Medical Graduates' Association. 1870. Edited by Leonard W. Sedgwick, M.D., Honorary Secretary. London: J. and A. Churchill. 1871.

35. Preliminary notice on the treatment of emphysema of the lungs by artificial expiration. By J. B. Berkart, M.D. London: J. and A. Churchill. 1871. Sewed.

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MEDICAL SCIENCE.

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PART I.

ORIGINAL COMMUNICATIONS.

ART. III.—*On Rheumatic Orchitis as a Sequel to Fever.* By
GEORGE F. DUFFEY, M.D., Dub.; L.K.Q.C.P.I., late Assistant
Surgeon 24th Regt.

THE frequency of the occurrence of rheumatic affections as sequelæ of the fevers seen in Malta, is a remarkable peculiarity of such diseases, and has been repeatedly noticed by the medical officers of the army and navy who have served on that station.

Rheumatic pains, or perhaps we should rather say, pains resembling those of rheumatism, are, it is known, frequent in the prodromal stages of many diseases; *e.g.*, the pains complained of in the bones and legs in typhus and enteric fever; in the back in small-pox; in the joints occasionally in scarlatina; in malarial fever, and at the commencement of the second stage of constitutional syphilis, &c.; consequently, it would appear (according to Dr. Aitken) that: "morbid poisons, the nature of which are yet undefined, give rise to a condition of parts which cannot be distinguished from rheumatism." But, with the exception of ague and relapsing fever, rheumatism, as a sequel of fever in the United Kingdom is, I believe, uncommon. What may be the reason for this peculiar sequel in the Mediterranean; whether it be an effect of the same unknown poison which primarily produces the fever—zymotic diseases and even rheumatism, being, in the opinion of some,

dependent on the sporules of a fungus circulating in the blood,^a—or whether it be induced by the depressing influence which the fever has exercised upon the system, causing a derangement of the assimilating processes, and therefore (as Prout's theory would lead us to believe), a conversion of the alimentary matters into lactic acid, I am unable to say; but in order to give a general idea of the nature of the fevers seen in Malta, it may be advisable to remark, that there are two main forms prevalent in the island. Firstly, ephemeral fever, or febricula, which is most common during the summer, and is attributable to climate: its usual duration is three days, but occasionally the pyreia is prolonged to a week (*ephemera protracta*), and succeeded by considerable debility. Secondly, a fever, or fevers, also of the continued type, described by different observers under a number of names;^b *e.g.*, "gastric remittent fever," "bilious fever," "Maltese fever," "common continued fever," "the long fever," "gastric fever," "typhoid fever," &c.; names which are in reality, I believe, after an experience of five years in Malta, one and all, varieties of the last, *viz.*, enteric fever; and I think that the terms gastric remittent, &c., have been originally applied to those anomalous and perplexing cases of enteric fever, which, indubitably, commonly exist, from an unwillingness and hesitation to give a peculiar type of a disease its proper nomenclature unless it actually comes up to, in every particular, the description of a normal case as laid down in the text-books, without making allowances for the difference which climate, and the predominance of certain complications and prominent symptoms must make on every disease. For example—my friend, Assistant Surgeon Boileau, who has given in the Army Medical Reports for 1866, a most comprehensive and excellent account of this "Mediterranean gastric remittent, and other allied fevers," accompanied by several graphic thermometrical observations, in relating what he calls a "doubtful" case, "intermediate between gastric and typhoid," says: that "this man was supposed to have typhoid fever. If he had, a man may be afflicted with it and have no spots on his skin; very little diarrhoea and the quotidian vacillations of temperature looked on as so characteristic of the defervescence of typhoid may be absent also." Again, Surgeon Charters has described an epidemic in the 100th Regt.,^c and acknowledges the difficulty of

^a The *Lancet*, 1867, p. 639.

^b Army Medical Reports, 1861, Dr. Marston: 1866, Mr. Boileau: 1865, Dr. Charters: *vide* also Naval Medical Reports.

^c Loc cit.

distinguishing "a prostrating, aggravated, and perilous fever" (in the autopsy of his single fatal case of which he found "abrasion of the apices of the Peyerian glands, which were somewhat more pronounced than usual, the mucous membrane of the cæcum congested, and the mesenteric glands enlarged and moist"), from "true typhoid;" but nevertheless describes the disease as gastric remittent. And Staff-Surgeon J. A. Marston, who has given^a the best and most scientific account of fever in Malta, while recognizing the prevalence of typhoid "with relatively slight symptoms," the rash also being "absent altogether in two-fifths of the cases treated," yet considers "gastric remittent" distinct from typhoid (with which he, not unnaturally, at one time confused it), mainly, it would seem, because of the predominance of gastric symptoms, the absence of diarrhœa and characteristic eruption, and comparative non-fatality of the former. As to the supposed dissimilarity between the diseases, due to the differences stated to exist in their pathological lesions, Dr. Marston, in the abstract of the solitary fatal case which he, like Dr. Charters, gives, states that there was congestion, with softening of the enteric mucous membrane, follicular disease evident in the cæcum, and hypertrophy of the liver and spleen.

Dr. Murchison, in his standard work on Fevers, says, that there may be no eruption in some cases of well-marked typhoid fever; and that the disease may be attended (as it was in my own person) with marked constipation of the bowels. I have also seen several cases in which, notwithstanding the presence of the "anatomical sign," there was no eruption during life. That the affection is not "remittent" has been shown by Boileau's thermometric observations, and corroborated by my own.

These protracted cases of the so-called gastric remittent fever, may then, I think, be fairly considered as enteric; as are also the frequent milder, and by no means fatal cases, which correspond, I believe, to the "abortive" typhoid described by Lebert, and recognized by Niemeyer.^b

It is, however, most difficult to draw any distinct line between either of these groups of fever, for, as Dr. Marston justly observes, they are all "congeners, forming interlaced links of the same chain," passing gradually from one into another.

Rheumatism as a sequel of both the ephemeral, but much more commonly the enteric fever, was chiefly observed as affecting the

^a Loc cit.

^b Text-book Practical Medicine, Vol. ii., p. 591, revised edition.

muscular and aponeurotic structures of the thigh, or back; the joints of the hip, knee, ankle, or shoulder; and occasionally the testicle. It is on this latter peculiar affection, that I purpose now offering a few observations.

The cases of rheumatic orchitis as a sequel to fever, of which I have notes, are 18 in number, and occurred, under my observation, in men of the 1st Batt. 24th Regt. (in which I had the honour of serving), while quartered in Verdala Barracks, at Malta, during the autumn and winter of 1867, and first two months of the following year. I also saw several cases in other regiments at the same time, and subsequently; but it is rather remarkable that there were no cases in my own corps the following years, and but few men in it, comparatively, suffered during convalescence from rheumatic pains, which were so frequent a sequel to the fever that prevailed in the garrison in 1866 and 1867, as almost to give it the character of a peculiar epidemic.

With four exceptions, all my cases of this rheumatic orchitis had been previously under treatment for fever. Some had been discharged, and were shortly afterwards re-admitted for the orchitis; others were attacked while still under treatment in hospital. Of the exceptions, two had had fever out of hospital a month prior to their admission for orchitis, and the other two, although no distinct former febrile attack could be traced, were both of a marked rheumatic diathesis. In none of the cases was there the slightest evidence or suspicion, of any previous gonorrhea, or other exciting cause; consequently, I would venture to define this affection as: an inflammation of the testicle of a rheumatic character, occurring sometime during, or a short time after, convalescence from fever. Drs. Marston and Charters both allude to its occurrence, during the convalescence of the fevers they have described in their respective reports.

The attack was generally sudden in its invasion. It was not uncommon, on going round the hospital wards, to hear from a convalescent from fever, who perhaps previously had—or had not, as the case might be—been complaining of “pains” in the hip, groin, or leg, that his testicle had become suddenly swollen and painful during the night. The swelling at first engaged the epididymis at the outer and back of the testis, and was accompanied by pain and fulness along the cord, and tenderness in the groin: the inflammation very rapidly extended to the tunica vaginalis; effusion into its sac took place, the scrotum on the affected side

became tense and distended to the size of one's fist; and its integuments assumed a shining appearance, with the veins prominent. During the first two or three days, sharp, pricking pains, running from the testicle into the groin, and sometimes shooting as far back as the loins, were complained of, with an uncomfortable sensation of a dragging weight and distension in the testicle, and there was considerable tenderness to the touch and heat of the parts. The local acute symptoms generally subsided after the third or fourth day, the only remaining indication, in the majority of cases, being the uneasiness occasioned by the weight: the scrotum could then be handled, and even compressed, without pain, the sensation communicated being that of a bag or bladder completely and uniformly expanded with fluid, very tense and hard. There were no general symptoms, such as pyrexia or nausea, but the acute pain, and sleeplessness often induced thereby, sometimes rendered the administration of an opiate necessary. The result of the inflammation was then, generally, a painless enlargement, of a chronic indolent character, and of an average duration of about a month—one patient was under treatment for eighty-five days—but if otherwise healthy, men were discharged to their duty as soldiers, without any detriment to them, whose testicles were, at the time, somewhat enlarged and indurated.

In one case (that of the hospital sergeant, a delicate cachectic man, who was much prostrated by a severe attack of fever), the right testicle became suddenly affected when he was in a very weak state during his convalescence: the inflammation pursued its usual course, there being the resulting chronic enlargement when the acute stage, which was rather prolonged, had subsided. Twenty-five days after the attack, severe pain was again complained of in the testicle, and on an examination deep-seated fluctuation was detected in the gland; the diagnosis of abscess was a few days afterwards corroborated by passing an exploring needle into the testis, pus being found in its groove on removal; an incision was then made, and exit given to a very large quantity of matter. Slight lipoma resulted, but the hernia was reduced by pressure and the application of pulv. hyd. nitric oxyd., and he made a good recovery. For some time subsequently he felt occasional twitchings in that testicle, apparently attributable to changes of weather, and the scrotum was thickened. Dr. Marston mentions that one of his cases also suppurated.

No evil consequences have been observed as a result of this

affection. In one case, that of a weakly old soldier, aged forty, who was subsequently invalided for chronic rheumatism, it appeared to have developed a slight tendency to varicocele. The right testicle was the one most commonly attacked; in eleven cases that being the side affected, and in five cases the left. In the remaining two cases both testicles were affected consecutively: one man had first the right testicle attacked and recovered; after an interval of six weeks the left was similarly affected.

Admitting that the affection is rheumatic, the cause of its occurrence cannot be attributed to metastasis, as it was occasionally the first and only local symptom for some time of the general affection; and this objection holds good to its being due to an extension of inflammation from contiguity of structure. In other cases, there was co-incident pain in the hip and knee-joints, and down the extensor muscles of the thigh of the same side as the affected testicle, and in one of the two cases in which there was no history of preceding fever, there was most acute sciatica of the corresponding limb. Many who had rheumatism after fever, but whose testicles were not affected, suffered (as I have had most unpleasant personal experience) excruciating pain in the groin, and spasmodic contractions of the muscles of the thigh. The special affinity which rheumatism has for fibrous textures naturally renders the tunica vaginalis testis liable to be affected by it; but why it should not be usually attacked in rheumatism elsewhere, either in the course of the acute disease, or as a sequel to fever, and was, so commonly in Malta, is, I think, remarkable.

The class of cases in which this orchitis was most liable to occur, was apparently in that of individuals of what are called a lymphatic temperament, and rheumatic diathesis, and whose constitutions had been impaired by a previous attack of fever. There did not seem to be any connexion between the severity or slowness of the febrile disturbance, and the liability to rheumatism, or rheumatic orchitis; nor was there any regularity in the time of the invasion of the latter after the defervescence of the fever, some cases occurring a few days after its subsidence, others not for a month or two.

The treatment adopted was various, and, I am bound to say, did not appear in any way to influence the duration of the complaint. In the acute stage, hot fomentations, with or without opiates, conium epithems, and cold evaporating lotions were employed. I think that the hot applications gave most relief. The testicle was always carefully supported, and in one case some of the scrotal

veins were lanced, but with no favourable result. In the chronic stage, strapping the testicle with the emp. ammoniaci cum hydrargyro, was the plan most frequently adopted, and in a few cases this was done, as recommended by Fricke, at an early period. Mercurial inunction, alterative doses of mercury, iodine, iodide of potassium, and colchicum were also tried; and as most cases, as I have previously observed, occurred in debilitated subjects, it was necessary to combine these remedies with bark, quinine, or cod-liver oil.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

On the Temperature in Diseases; a Manual of Medical Thermometry.

By Dr. C. A. WUNDERLICH; translated from the Second German Edition by W. BATHURST WOODMAN, M.D., New Sydenham Society. London, 1871. Pp. 468, with woodcuts and lithographs.

AMONG the many improvements recently added to our implements of medical diagnosis, perhaps none deserve our attention so much as the clinical thermometer. There is little doubt that clinical thermometry, carefully employed and scientifically interpreted, must rank next, if not equal to auscultation as a system (for a system it has now become) of interpreting the phenomena of disease.

Medical thermometry has been too much neglected in British practice, not, we believe, from any carelessness on the part of our practitioners, who are as ready as those of any other country to adopt means of diagnosis likely to benefit their patients, nor from an apathy on their part to follow up and improve on the observations of their Continental brethren, but from the difficulties in the way of informing themselves of what has already been done on the Continent.

Until Dr. Aitkin placed systematically, in his work on the practice of medicine, the thermometric phenomena of acute disease among the symptoms to be regularly looked to as of scientific and practical import, our practitioners had given but little general attention to the accurate noting and interpreting of the heat of the human body in disease. Although the work of Aitkin, as it first appeared, had many imperfections and even errors in its description of thermometric phenomena, yet it served the valuable purpose of stimulating hundreds of inquirers (ourselves among the number) to commence systematic thermometric observations. To Dr. Aitkin all members of the profession and, especially, medical thermometricians, owe a debt of gratitude for thus—at a time when no

systematic treatise on medical thermometry was in existence—placing the chief points of this department of diagnosis before them. The stimulus thus given by Dr. Aitkin has already produced good fruit, not only in the way of substantial scientific and practical work, but also in preparing our profession for the reception of the valuable work now before us in English dress, thanks to the New Sydenham Society, and their careful editor and translator, Dr. Woodman, and the kind assistance of the original author, Dr. Wunderlich, who probably would have done more, but that he was engaged in the heavy and patriotic duty of superintending three military hospitals during the late Franco-German war.

As Laënnec was the father of, though not originator of auscultation, so Wunderlich must be considered the father of medical thermometry, though not the first to employ it as a means of diagnosing disease. His treatise will be looked to, in ages yet to come, as the real foundation of medical thermometry, and while there have been many observers and writers, their observations, while always duly acknowledged by our author, and often criticised, are seldom taken as positive evidence without full investigation, and careful comparison with observations conducted by the author or his assistants. In his preface, Dr. Wunderlich states that for sixteen years his attention has been directed to this subject, and he has “got together many thousand complete cases of thermometric observations of disease, and millions of the separate readings of the temperature.” It is scarcely necessary to point out the reasons why the thermometry of disease is of importance, but we may mention a few of the more important. “It can neither be feigned nor falsified; we may conclude the presence of some disturbance in the economy from the mere fact of altered temperature; the height of temperature often decides the degree and danger of the attack; certain degrees indicate most rapidly and most safely any deviations from the regular course of the disease; it reveals the existence of complications, and shows how far recovery is from being complete, &c.” Two important points have to be noted at the outset of inquiries into temperature. 1st. The constancy of healthy temperature under nearly all conditions and climates, namely, 98.6° to 99.5° Fahr. in the closed axilla, with a variation of seldom more than $.9^{\circ}$ Fahr. in twenty-four hours. 2ndly. The more constant the temperature, the more healthy the individual; though a normal temperature does not necessarily indicate health, all those whose temperature either

exceeds or falls short of the normal range are unhealthy. Certain limits are rarely exceeded in the temperature range of disease. The highest authentic temperature recorded during life, is 112.55° , but the most severe diseases range between 95° and 108.5° Fahr. and very seldom exceeds 109.4° Fahr., or sinks below 91.4° Fahr. Our own experience leads us to believe that temperatures above 106° Fahr., are uncommon. "A normal temperature in sickness is only to be considered as a relative sign," but never, by itself, leads to positive diagnosis. A rise in temperature to 101.3° Fahr., or more, is usually accompanied by subjective feelings of heat, lassitude, &c., followed by loss of body-weight, and amounts to fever. Many separate kinds of disease correspond to *well marked types of altered temperature*. These answer to well marked varieties of disease. The author classifies his cases into those belonging to the "typical states of disease," as typhus, typhoid, exanthemes, pneumonia, and intermittent fever. "Approximately typical forms," which occasionally deviate widely from their usual types, as tonsillitis, parotitis, cerebro-spinal meningitis, acute tuberculosis, &c.; and an irregular group (which we agree with the translator in expressing disapproval of), including diseases not usually febrile, yet, when accompanied by fever, assume a regular type of temperature range. A considerable amount of valuable information may be obtained by a single observation, but a single observation, while giving valuable indication of the patient's condition, is "not, by itself, conclusive as to the kind of disease." Wunderlich gives the following useful classification of the information to be derived from single observations:—

"A. *Temperatures much below normal (collapse temperatures)*, below 98.8° Fahr.

(a) Deep, fatal algide collapse, below 92.3° F.

(b) Algide collapse, 92.3° to 95° F., indicates great danger not compatible with recovery.

(c) Moderate collapse, 95° to 96.8° F., in itself without danger.

B. *Normal or almost normal temperatures.*

(a) Sub-normal, 96.8° to 97.7° F.

(b) Really normal, 97.88° to 99.12° F.

(c) Sub-febrile, 99.5° to 100.4° F.

C. *Febrile temperatures.*

(a) Slight febrile action, 100.4° to 101.12° F.

(b) Moderate degree of fever, 101.3° to 102.2° F., in *morning*, rising to 103.1° F. in evening.

(c) Considerable fever, about 103.1° F. morning, and about 104° F. in evening.

(d) High fever above 103.1° F. morning, and 104.9° F. evening

D. *Temperatures which in every known disease except relapsing fever in all probability indicate a fatal termination, 107.6° F. or more (hyperpyretic temperatures.)*"

The above will furnish a ready measure of the danger of a patient at any visit, as far as a single thermometric observation can go, but in order to follow a case, and ascertain its deviations from the normal course of the disease, we must not only have one, but in many cases, several daily observations, and in some instances even hourly observations are necessary.

"*Continual observations of temperature, repeated several times a day through the whole course of a disease, or for a considerable period of its duration, afford the best materials for diagnosing and prognosticating the nature and results of any disease, when this is associated with considerable elevations of temperature we learn from them what is conformable to law or normal in the course of febrile diseases, thus gaining a solid basis for diagnosis in individual cases. They may often, in themselves considered, afford a perfectly correct diagnosis of the kind of disease; or, to say the least, they furnish the most important and trustworthy materials for a diagnosis, and sometimes are the only possible means of deciding in doubtful cases.*" The foregoing may appear to some a rather enthusiastic laudation of the diagnostic powers of the clinical thermometer, but our own experience leads us to believe that its value is not here much if at all over-rated.

In the course of febrile diseases, we may distinguish the following stages or periods in the range of temperature:—

A.—*Periods preceding the Termination of the Disease.*—1. The period of development (pyrogenic stage), which must be considered at an end with the development of a localized process, or where the lowest average daily temperature characteristic of the disease is reached.

2. The period of full development of the disease (the acme, the fastigium), during which the characteristic daily elevations of temperature are maintained.

3. The period of perturbation (amphibolic), usually follows in some cases, in which the temperature shows a more or less irregular course.

B.—*Periods in Cases which Recover.*—1. The crisis (perturbatio critica, or stage of decrement), or period of decided but as yet insufficient decrease.

2. Period of return to normal temperature (defervescence).

3. The epicritical period, and period of convalescence, in which the temperature is normal, or below normal, or sometimes a little above normal.

C.—*Periods of Fatal Termination*.—1. Pro-agonistic (preceding death struggle), during which the peculiarities of the temperature or other circumstances point out the commencement of a fatal termination.

2. The death struggle.

3. The act of dying and *post-mortem* changes of temperature.

We have thus indicated, as our space permits, the “fundamental principles of clinical thermometry,” and done this as much as possible in the words of Wunderlich’s translator.

The history of clinical thermometry is of much interest, but we have not here the space to do more than mention one or two of the main points.

Sanctorius, who died about 1638, was the first to attempt an *exact* measurement of the temperature in disease. Of course the heat of the body in fever was well known to the ancient physicians. Sanctorius also relied much on alterations of the body—weight of the patient as a means of diagnosis, and considered that alterations of temperature and weight were the main points to be determined when estimating the progress of the patient. About 1740, to Martin, an Englishman, is due the first publication of anything like accurate thermometric observations. We then find among medical thermometricians, many of the great names of the last and (early part of) present century.

In 1797, we find James Currie’s “*Medical Reports on the effects of water cold and warm*, as a remedy for fever and other diseases,” in which he records the effect of the treatment by these agents in altering the temperature of the patients. From about 1840, up to the present, thermometry has gradually attained a sounder position in scientific diagnosis. This must be attributed almost entirely to the observations of German physicians, although many, but not very systematic observations were made in other countries; and among our own countrymen we may mention the names of George Kennedy (who took large numbers of thermometric observations in fever), and M'Donnell of Belfast.

Barensprung and Traube were the first to place medical thermometry on anything like a systematic basis, and Wunderlich

informs us that it was the observations of Traube which induced him, in October, 1851, to commence the observations, by the accumulation of which the author has been able to arrange a system of clinical thermometry.

The bibliographic sketch contained in the original work, does ample justice to clinical thermometricians of all countries, up to the date of the publication of the second edition, and the translator has, by a "supplemental bibliography," brought the subject up almost to the day of publication of his translation.

The remarks on "The Art of Medical Thermometry" (chap. 4), are excellent, and will afford much encouragement and comfort to those who think that the time, trouble, and accuracy necessary in the practise of medical thermometry, are insurmountable obstacles to its general use.

A thermometer with a bulb $\frac{1}{10}$ to $\frac{1}{4}$ of an inch in diameter, globular or nearly so, glass neither too fragile nor too thick (so as to impede conduction of heat), graduated to fifths ($\cdot 2$ C.) of a degree C., which can easily be divided by the eye into halves or quarters ($\frac{1}{10}$ to $\frac{1}{20}^{\circ}$ Fahr.), range of graduation from $90^{\circ} 5'$ to 113° Fahr., is the form recommended. Registering maximum thermometers are not insisted on in the original text; but the translator, in notes, refers more particularly to these, mentioning the form introduced by Aitken. The note states that Messrs. Harvey and Reynolds, of Leeds (at Dr. Albutt's suggestion), were the first to make a six-inch registering clinical thermometer. Without laying any specific claim to priority in this matter, we may mention that having purchased a set of Dr. Aitken's thermometers immediately after their introduction (of Mr. Yeates of Grafton-street, in this city), the idea of reducing their size at once suggested itself, and Mr. Yeates in a very short time produced for our use two excellent clinical thermometers, but slightly over six inches in length. The propriety of testing the accuracy of the thermometers from time to time is very properly insisted on. We may mention, that not long since, we saw two thermometers in the same case differing from one another to the extent of a whole degree, and one being at least two degrees (Fahr.) out (too high). Our own experience leads us to believe that the best form of thermometer is a registering instrument about six inches long, graduated from 90° to 110° or 115° Fahr., each degree divided into fifths. We should prefer the use of the centigrade scale, but so long as the present forms of weights and measurements are adopted in these countries, we despair of introducing

anything like systematic decimal division in any matters chiefly meant for practical purposes.

The place chosen for taking the observation is the closed axilla, the mouth, rectum, and vagina being all rejected as unsuitable for many reasons. The time for leaving the thermometer *in situ* is described "in general practice, until two or three minutes after it has appeared to stop rising." We generally leave the instrument for five minutes in the axilla, then note the temperature, examine again in not less than two minutes, if no additional rise has taken place in that time we consider we have the patient's real temperature, if not we repeat the test at similar intervals until the mercury seems steady.*

In considering the question, "by whom shall the observations of temperature be made?" Wunderlich makes the following remark:—

"So long as only one or two daily observations are taken, even in some cases, this objection is of little value, since almost any medical man can afford to spare a couple of quarter hours, for very acute cases, which do not occur so very often in private practice. If he cannot spare this amount of time he had better not undertake the case. The logic of this objection is rarely better than that of an accoucheur, who would not wait to the termination of a difficult labour he had undertaken, for want of time!"

The above may read somewhat differently to Englishmen and German practitioners, but this we leave to our readers.

"The causes of altered temperature in disease are very numerous and complicated (there being seldom, if ever, but *one* cause in action at a time), and do not depend on their increasing or diminishing either the production or the loss of heat; but rather on the fact that the regulating force is less perfect than in health.

"It is quite conceivable that the defective balance of many functions which compensate each other in health may arise from very different sources."

The loss may be so great that the greatest rise in formation possible to the individual may not compensate it; the giving off may be so hindered in the bulk that with a limited production of heat an accumulation of temperature is certain. The production

* By watching a thermometer *in situ* continuously for about twenty minutes, we have seen the mercury rise and fall appreciably during that period. This is a point to be especially mentioned when considering temperature in relation to the therapeutic action of drugs, particularly stimulants.

may be so great that all the contrivances for carrying it off are of no avail and the temperature increases, and besides in disease we have "other sources of heat production unknown to the healthy body," as rapid destruction of tissue and sources of diminution, as effusions of fluid, &c.

"The primitive causes which may lead to morbid changes of temperature or modify those existing may be

"(a.)—External influences.

"(b.)—The circumstances and constitution of the individual.

"(c.)—The processes going on in the organism itself."

Experiments on animals have shown how many of these causes may come into action; but our author gives us a caution, which it would be well were it more attended to when drawing conclusions from experiments on animals. "Human beings exhibit slighter normal fluctuations of temperature than most other animals." Considerable thermometric variations are produced in rabbits by merely tying them down for experiment. A similar caution is uttered as to arguments from experiments on healthy men; these warnings are still further enforced by the translator in his notes. While we thoroughly agree with both author and translator on this question, we hope we will not be understood to deny the utility of well conducted physiological experiments; but we gladly take this opportunity of protesting against the rash conclusions that have recently been drawn with regard to the action of certain drugs; conclusions diametrically opposed to clinical experience, and drawn solely from the results of experiments on the lower animals. The influences which operate on depression of temperature, do so by abstracting heat from the body, by limiting the access of warm blood to the parts under observation, or by diminishing the production of heat. Extreme degrees of external cold are the most certain means of abstracting warmth from the body, even so far as to make death certain. "The effects of cold drinks and cold injections on a morbidly elevated temperature are transient, and indeed rapidly so; constant washings, &c., are far more effective. Unlike these, the effect of water more or less cold in the form of packing (*Einwicklungen*), in wet sheets, of full baths and douches is far more considerable, intense, and permanent." The above quotations point to the value of a judicious application of cold in the treatment of febrile diseases, upon which the author has some valuable remarks, concluding thus:

"It is no doubt an error to believe that the benefits derived in cases of fever depend simply upon the mere abstraction of a detrimental (over) plus of heat.

"Temperatures above blood heat, or even approximating closely to this, have, when long continued, a decidedly morbid influence, and cause the temperature of the body to rise."

Sunstroke may be considered as an example of this, and Wunderlich proves that during excessively hot weather there was an unusual increase in the temperature in his patients. This is what we should expect from the ease with which the temperature of a sick person is affected; we have on several occasions taken uncommonly high temperatures (in not very severe cases of typhus), in very hot weather, and we believe the cause was due to the influence of external temperature. "The application of external irritants appears to have the effect of lowering the temperature rather than raising it." Large losses of blood cause a rapid lowering of temperature in health and disease. "General blood-letting in suitable cases of disease have similar effects, and it not seldom happens that the temperature, which just before was considerably elevated, becomes normal or nearly so just after." This of course will explain the supposed benefit of blood-letting in fevers as practised in by-gone days; but the following remark points in the opposite direction:—"But the reaction is generally far from insignificant. In most cases the temperature soon rises again to its previous height or often exceeds it." Sometimes with the first enjoyment of animal food, during convalescence, the temperature rises as much as 3.6° F., which might lead to the belief that a relapse was impending, particularly as it may remain at this height for several days. The following medicines are mentioned as lowering fever temperatures:—Digitalis, veratria, quinine, antimony, and calomel; acids and salines do so less effectually. The following agents introduced into the system raise temperature:—Coffee, musk, camphor, curare, putrid solutions or pus (injected subcutaneously or into vessels), or blood of a fever patient (healthy blood does not).

"*Muscular exertions* generally cause a very considerable rise in temperature in cases where there is any previously existing morbid condition, however slight. On this account *we are quite* justified in feeling anxiety about the health of any one whose temperature exceeds the normal after only moderate exercise, however cheerful and apparently well he may seem in other respects;" but the temperature of a patient who has recently been removed (as previous

to admission into hospital) must not be relied upon as a true indication of his state, although of course, great alteration of temperature under such circumstances would indicate easy variability, and therefore considerable disease, in accordance with the foregoing rule with regard to exercise. Temperature in females and children is more easily influenced than in males generally; in males of advanced life the alterations are rather slow, and in acute diseases lower than in younger people under similar circumstances.

The following are definite and characteristic types of altered temperature:—(a) Rigors (feberfrost), (b) fever heats or pyrexia (feberhitze), (c) collapse. These are distinct pathological states, not mere variations in temperature, for these are accompanied by other definite symptoms. During a rigor the body temperature is generally increased to 40° C. (104° Fahr.), or more, while the extremities become cooler than normal, and there is in spite of the *real* increase of temperature a sensation of chilliness—in fact, we should find it hard to make our patient believe that he is really warmer than in health. This is the rigor of a commencing fever, followed by the regular fever-heat. A rigor may, however, occur without any appreciable rise in temperature; such occurs frequently in nervous people, and is of little moment. Nervous rigors may occur from any sudden impression on a sensitive part, as passage of a catheter. Rigors may also occur with falling temperature, but do not seem intimately connected with collapse temperatures. These three forms of rigors point to the importance of thermometric observations after severe surgical injuries or operations, where a true rigor (*i.e.*, with a rising temperature) is often of such serious import.

The fever heat consists in a rise of from 2° to 3° C. (3.6° to 5.4° Fahr.) or more; there may not be any subjective phenomena, as thirst, langour, or quickness of pulse, but these are usually present, and are followed by loss of body weight, constituting true fever. Wunderlich in this connexion “*emphatically asserts that there is generally no exact parallelism between the height of the temperature on the one hand and the kind and degree of the remaining phenomena on the other, &c.*” In another part of the work the nature of these relations is more closely considered and commented on. This frequent want of correspondence between temperature and other symptoms has been often used as an argument against the value of thermometric observations, while on the other hand a supposed correlation of other symptoms with temperature has often

led to false conclusions, especially in prognosis. "Collapse sometimes occurs by itself, sometimes in the middle of pyrexia of various degrees; very often in the *sequelæ* of fevers; very seldom during rigors, although these have many phenomena in common with collapse. . . . Collapse is more transient and less eventful than pyrexia, or even rigors, and when it is relatively protracted it still forms only an episode, or the last act (of a drama), of relatively short duration." Collapse varies in its degree from slighter forms, without any particular complaint on the part of the patient, but slight alterations in appearance, and some coldness of nose, cheeks, hands, feet, &c., to the most extreme degree where the patient becomes as a corpse, with sunken appearance, intense coldness, icy sweat, extreme weakness, anxiety, giddiness, &c. The danger of collapse seems more to depend on the cause inducing it than on the state itself. This form of collapse generally occurs suddenly, but collapse occurring in acute febrile disease begins slowly, and the collapse symptoms are gradually established. While, however, the extremities seem cold, the body temperature may be increased, normal, or diminished, the usual condition being a rapid fall up to even 10° or 14° Fahr. It will thus be seen that no distinction can be made between rigor, fever heat, and collapse from the mere height of temperature. Rigor and collapse must be distinguished by the former occurring with a rising, the latter with a falling temperature.

With regard to the relation of temperature to other symptoms, there seems to be too general a belief in their close correlation. "There is very often a contrast between the temperature and the frequency of the pulse." Slight febrile temperature usually coincides with a pulse of 80 or 90, moderate fever 90 to 108, and considerable fever from 108 to 120, but these numbers have only approximate value. The temperature is more closely related to the pulse than to other symptoms. There is sometimes slight correspondence between the brain symptoms and temperature, being more intense in proportion to the temperature in old persons and children than in adults. Daily fluctuations of temperature are almost always present in disease, and may depend upon a variety of causes, which are classified as depending on (a.) the elements which constitute the morbid process—kind, intensity, stage, regularity or irregularity, or other peculiarities of its cause, improvements or relapses, complications or special events, progress towards health, or fatal crisis; (b.) idiosyncrasy of the patient, intercurrent

external influences, or therapeutic agents. "The *daily difference* may vary greatly, and the significance of the same number of degrees may be very different in proportion as the daily mean is at the same time high or low." In a daily mean of 37° C. (98.6° F.), a daily excursus (variation) 1° C. (1.80° F.) is of no importance; if it amounts to 1.5° C. (2.7° F.) it is suspicious. When the daily mean reaches 37.5° C. (99.5° F.), the foregoing variations indicate the presence of disease of some sort. If the daily mean is up to 38.5° C. (101.3° F.) or more, the daily difference is of higher importance. If less than $.5^{\circ}$ C. ($.90^{\circ}$ F.), indicates a continued, less than 1° C. (1.8° F.) a sub-continued, and when more considerable, an intermittent fever. If the daily minimum reaches the normal, there is an intermission; but real intermission only occurs when *all* the symptoms of fever abate. As a rule, there is but one exacerbation in twenty-four hours.

"Febrile diseases exhibited great variety as regards the course pursued by the temperature; but, in spite of all these differences, we can recognize certain rules as regulating their behaviour, and it is also true that the very differences they exhibit furnish us with the most important data for distinguishing the several forms of disease and their varieties." A temperature continuously above normal indicates a *continued fever*; where interrupted by an apyretic period (or periods) indicates an intermittent or relapsing fever. The course pursued by temperature may be influenced by the nature of the disease, its intensity by individual circumstances, accidental circumstances ("including many therapeutic undertakings!") or complications. We have already referred to the three stages of febrile diseases, the pyrogenic, fastigium, amphibolic (pending the termination), defervescent and epicritical (in cases of recovery), and pre-agonistic and agony (in case of death). The pyrogenic may be either short or protracted; in the first form rising suddenly and steadily, or by but slight interruptions, and reaching its height in from a few to thirty-six hours; the heat of the trunk rising much more rapidly than that of the extremities. This kind of pyrogenic stage is the rule in small-pox, scarlatina, true pneumonia, malarial attacks, pyæmia, and relapsing fever, and commonly in typhus, febricula, facial erysipelas, tonsillitis, and meningitis of convexity of the brain. It never occurs in typhoid, basilar meningitis, catarrhal affections, or rheumatic fever. The protracted form of pyrogenic stage may last for several days or a week, of course with daily variations. This

form occurs most commonly in typhoid fever, and may form a basis for diagnosing that disease. This type is also commonly found in measles, bronchial catarrh, catarrhal pneumonia, basilar and cerebro-spinal meningitis, acute tuberculosis, rheumatic fever. In some cases of rheumatism, secondary syphilis, phthisical affections, and serous inflammations, the temperature rises in an insidious manner, and can scarcely be classed with any type. The height of the temperature during the fastigium depends on the severity and nature of the disease ; the daily mean is of more importance at this period than the absolute height, the average height of the whole fastigium being more symptomatic than that of single days. The following is the average height of the fastigium in the more important febrile diseases:—

Typhoid fever, from 39° to 40.2° C. (102.2° to 104.36° F.) Typhus, 39.2° to 40.5° C. (102.56° to 104.9° F.) Small-pox (eruptive fever), 39° to 40° C. (102.2° to 104° F.) Measles about same as last. Scarlatina, about 40° C. (104° F.) True pneumonia, 39.2° to 40° C. (102.56° to 104° F.) Articular rheumatism (uncomplicated), 38° to 39.5° C. (101.3° to 103.1° F.) Influenza, 38.5° to 39.2° C. (101.3° to 102.56° F.) Facial erysipelas, 39.5° to 40° C. (103.1° to 104° F.) Parenchymatous tonsilitis, about 39.5° C. (103.1° F.) “The most valuable data for both diagnosis and prognosis are obtained from the general course of the temperature during the fastigium.”

The course of temperature during the fastigium is (when expressed in the graphic form, on a chart) of one of three principal types:—

1st. What Wunderlich calls “the acuminate form,” that is, rapidly reaching its highest point, from which it rapidly begins to fall, or terminates fatally, thus giving an acute angle on the chart. This form occurs in all ephemeral fevers, paroxysms of intermittents, daily attacks of fever in tuberculosis, in some fevers of several days’ duration, and in terminal fevers.

2nd. A continuous course of temperature during the fastigium, with but slight fluctuations. This occurs in all cases of severe, acute disease, in some complications supervening on previous disease, and also in some mild, long continued fevers. In this form a considerable dip in the chart may sometimes occur, and give rise to an apparent crisis (“pseudo-crisis”). This continued course generally lasts less than a week. The diseases in which this form occurs are typhus, scarlatina, true pneumonia, first stage of small-

pox, facial erysipelas, tonsillitis, meningitis of convexity of the brain.

3rd. The non-continued form, which shows great ups and downs on the chart. This is the most common form, and to it belong typhoid fever, measles, catarrhal affections, rheumatic fevers, osteomyelitis, basilar meningitis, pyæmia, secondary small-pox fever, tuberculosis, &c.

Of all diseases which exhibit a non-continuous course in their fastigium, typhoid is the most clearly typical. The close of the fastigium is sometimes defined, but may pass gradually into the succeeding stage. Sometimes a slight rise (*preturbatio-critica* of older physicians) occurs at the close of the fastigium.

The course of temperature during progress towards recovery may be divided into—(a) the stage of insufficient decrease; (b) defervescence (or cessation of fever); (c) epicritical period (recovery). The decrease may be either by lysis or crisis—in the former the temperature falling gradually, in the latter falling suddenly. Relapse, or the occurrence of sequelæ after defervescence, is indicated by a sudden rise of temperature; but we must not forget, in estimating the value of these, our previous remarks about the effect of too sudden return to ordinary diet.

It would be impossible to go into the consideration of the chapters on the temperature in special diseases, including as they do almost all the febrile affections. The disease most carefully treated of in this part of the work is typhoid (abdominal typhus). That on typhus (exanthematic typhus) is rather slight, the author having had but comparatively slight opportunities of observing this affection. All the other chapters on special diseases are excellent, as also that on chronic diseases. The work also contains a very full account of the physiology of healthy temperatures, and the author discusses many other interesting theoretical questions with his usual ability, but our object has been to touch upon the general, practical bearings of the work. In fact, the only fault to be found with Woodman's edition of Wunderlich is that it is too good, and we are afraid that comparatively few will follow it into all its windings. We regret that, being a Sydenham Society book, its circulation will be limited.

THOMAS WRIGLEY GRIMSHAW.

The Dynamics of Nerve and Muscle. By CHARLES BLAND RADCLIFFE, M.D., F.R.C.P., Physician to the Westminster Hospital and to the National Hospital for the Paralysed and Epileptic. London: M'Millan & Co. 1871. Pp. 288.

It is more than twenty years since Dr. C. B. Radcliffe had his attention first directed to the subject of which he treats in the work before us. During this period his opinions regarding the physiological relations existing between nerve and muscle have undergone considerable change. This change is, in part, due to re-consideration of facts which were before him from the first, but also, in great part, to the accumulation of new observations, made with the aid of Sir William Thomson's new quadrant electrometer. Dr. Radcliffe, in his new volume, frankly admits that his former attempts to solve some of the more abstruse problems connected with the dynamics of nerve and muscle were very imperfect. Of his first little work he speaks as Dryden did of one of his plays—"It was," he says, "only a confused mass of thoughts tumbling over one another in the dark, when the fancy was yet in its first work, moving the sleeping images of things toward the light, there to be distinguished, and then either chosen or rejected by the judgment." Nor does the author speak more approvingly of his later efforts—not even of that the latest of all, which took the form of lectures delivered at the Royal College of Physicians of London in 1863.

Dr. Radcliffe early perceived that the true key to the dynamics of nerve and muscle lay hid among the facts belonging to animal electricity. He devoted himself with great earnestness to the study of the fascinating investigations of Du Bois-Reymond; he carefully scrutinized the muscle and nerve-currents, and the changes which take place when they are passing from a condition of rest into one of action. Gradually he began to apprehend that there was another aspect of animal electricity, which is passed by unnoticed in the researches of Du Bois-Reymond. His attention became fixed on the discovery of Matteucci, that muscular contraction was accompanied by a discharge analogous to that of the torpedo.

This discovery seemed to throw a new light on the matter. It seemed to point, not so much to the muscle-current and nerve-current as fundamental conditions of animal electricity, but rather

to a state of *charge*. He began to seek for evidences of this condition of charge, first with one electrometer, and then with another, first with one condenser and then with another, and even at first not altogether fruitlessly. It was only, however, when he began to work with Sir William Thomson's new quadrant electrometer, along with apparatus for measuring the resistance which the animal tissues offer to electrical conduction, that he began distinctly and thoroughly to realize the facts with which he had to do. By experimentation with these instruments he became convinced that, for all practical purposes, the animal tissues may be regarded as non-conductors. He found, for example, that the resistance offered by an inch of the sciatic nerve of a frog was not less than 40,000 B. A. units—as much, that is, as eight times that of the whole Atlantic cable. Dr. Radcliffe likewise found, when seeking for tensional phenomena of animal electricity in muscle and nerve, by means of the new quadrant electrometer, that the sides and ends of the fibres were charged differently, the former positively, the latter negatively, and that these evidences of charge disappeared, in a great measure, during action. He found evidences of the charge, for which he had formerly sought almost in vain; but he found more than he expected. Expecting to find a single charge he found a double one: here was a difficulty not hitherto anticipated. In fact, it was a difficulty so great as to be absolutely insuperable on the hypothesis adopted by Dr. Radcliffe in his earlier works.

His idea had been that the muscular fibres were *charged* with *one* kind of electricity during rest, and that in this way the molecules were kept in a state of constant repulsion. This idea was absolutely irreconcilable with the fact that there is a double charge of electricity in muscle. It was one which agreed well enough with the presence of a single charge, whether positive or negative; but it clashed altogether with the new state of facts, seeing that with a double charge the mutual repulsion of the molecules, arising from the presence of either charge, would be counteracted by the mutual attraction of molecules charged differently.

Besides the discovery of the double charge, it is to be remembered that the author had also come to recognize more fully than in former years the true character of the animal tissues as non-conducting: hence the questions naturally arose—was it possible that the sheaths of the fibres in muscle might be so wanting in conductivity as to allow them to act as *dielectrics*? Was it

possible that, the sheaths being dielectrics, a charge of one kind of electricity developed on the outsides by the reaction of the blood then circulating, or in any other way might induce a charge of the other kind of electricity on the insides, and that the electrical antagonism of the sides and ends of the fibres might be accounted for by the charge induced on the insides being conducted to the ends by the contents of the sheath? Was it possible that the fibres might be kept in a state of relaxation or elongation by the compression of the sheaths, arising from the mutual attraction of the two opposite charges disposed as in a charged Leyden jar, upon the two surfaces of the sheath?

These were the questions which, in turn, presented themselves, and which, as it seemed, required to be answered in the affirmative. The theory of electrical action in muscle thus foreshadowed involved an idea quite distinct from that whose place it had taken. The new idea was definite, and free from many objections which might reasonably be urged against the former one. It accounted for the difficulty of detecting the charge present in the fibre, and for the fact that the fibre could keep its charge though itself uninsulated: for the two charges disposed thus, after the manner of an elongated Leyden jar, upon the two opposite surfaces of the sheath of each fibre, marked each other, and, at the same time, imprisoned each other, just as they do in the ordinary charged Leyden jar.

But what is by far more important, in support of Dr. Radcliffe's later notions, his new idea of the relationship of these electrical phenomena in muscles was definite, is this—that all the tensional phenomena of the muscular fibre, and all the current phenomena also, could be imitated upon a wooden model of the fibre left bare at the two ends, and sheathed at the sides with a coating formed of two layers of tin-foil, separated by an intermediate layer of thin gutta-percha sheeting, if only a charge was supplied to the outer layer of tin-foil. And in this also was the idea definite, that the elongation of the fibre, assumed to be brought about by the mutual attraction of the two opposite charges, disposed (as in a Leyden jar), upon the two surfaces of the sheath, was found to be reproducible on a narrow band of india-rubber, covered, on its two surfaces, with a thin metallic coating, so as to allow of its being charged as a Leyden jar is charged; for on thus charging, this band was seen to elongate, under the mutual attraction of the two charges disposed upon its surfaces, just as the sheath of a

muscular fibre is supposed to do. Nay, by this artificial arrangement everything which, according to Dr. Radcliffe's present theory, is supposed to happen in muscular motion, can be fully illustrated, contraction as well as relaxation, for this band which had elongated under the charge is seen to contract when this charge is discharged.

In fine, the result at which Dr. Radcliffe has arrived is, that the workings of all kinds of electricity upon muscle is resolvable into those of charge and discharge, the charge elongating the fibres, and the discharge of this charge bringing about the state of contraction.

Such is a brief sketch of the new view which Dr. C. B. Radcliffe now sets forth as to the dynamics of muscle and nerve. To comprehend his facts and arguments thoroughly his work must not only be perused, but perused with great care. The book is beautifully brought out, and although we are unwilling to pronounce a decided opinion upon views which, if accepted, would so profoundly modify generally received opinions as to the physiology of muscular action, yet we do not hesitate to say that Dr. Radcliffe's work is a most remarkable and suggestive contribution to the literature of the subject.

WORKS ON ELECTRICITY.

1. *Lectures on the Clinical Uses of Electricity.* By J. RUSSELL REYNOLDS, M.D., F.R.S. London: F. & A. Churchill. 1871. Pp. 112.
2. *De l'Électrisation localisée et de son application à la pathologie et à la thérapeutique.* Par le Dr. D. B. DUCHENNE (de Boulogne). 3ème édition; 1re partie. Paris: J. B. Baillière et Fils. 1872. Pp. 576.

DR. REYNOLDS has, we think, acted wisely in revising and publishing, in a separate form, these admirably concise and practical lectures, which were originally delivered in University College Hospital. The extent to which even the best text-books are overladen with debatable matter, crude theories, and idle speculations is a serious hindrance to all who endeavour to ascertain what is truly known, and it is quite refreshing to have this important subject handled clinically by one so competent to speak dispassionately, both

from his general attainments and special experience in this branch of medical art. It is re-assuring to learn, and in this we heartily follow the author, that, for the relief of pain or spasm, it is perfectly immaterial whether we employ a "descending" or "ascending" direct galvanic current, though theoretically it should make a very great difference; and that, in the case of induction machines, notwithstanding Duchenne's assertions, it matters little, except so far as intensity goes, whether we use the *primary* current (*i.e.*, current from the coil of thick wire), or the *secondary* current (*i.e.*, current of the fine wire). After a few introductory remarks on the clinical uses of electricity, and on the different forms of electricity in medical use, the author briefly exposes the more prominent effects of electricity on the vital properties of the tissues, the diagnostic uses of electricity are more fully treated of, a subject most unjustly ignored in ordinary practice, and the concluding chapter discusses the therapeutical uses of electricity, and its method of employment in paralysis from cerebral, spinal, or local disease; in spasmodic affections; and in neuralgia. Dr. Reynolds' short treatise is a trustworthy handbook of practice, in outline, and conveys its teaching in a very lucid manner; but it should be remembered, that no one ought to meddle with electro-therapeutics until he has gained some knowledge of the rudiments of electro-physics, and has learned for himself the practical management of some one of the forms of galvanic and faradic apparatus, respectively.

Within the last two years we have, from time to time, noticed in this Journal the valuable works on medical electricity produced by Althaus, Meyer, and Messrs. Beard & Rockwell, and we now welcome with pleasure the third and revised edition of Duchenne's great work.

Owing to the events of the late war part of this edition has already appeared in an English dress, under Dr. Tibbits' care, before the original could be given to the world, and the remaining part of the English translation and the second volume of the French issue will, probably, be soon in circulation. Duchenne, by his unwearied and protracted investigations, has well earned his position as head of the modern school of electro-therapeutics; and, as has been remarked, his voluminous work is not only a well-nigh exhaustive treatise on the uses of electricity, but it is also an elaborate exposition of the different diseases in which electricity

has proved to be of value as a therapeutical and diagnostic agent. The arrangements of the contents has always seemed to us intolerably confused, much needless repetition and prolixity are indulged in, and the ever-recurring tone of egotism is offensive; yet, to all who will bear with these drawbacks, the labours of Duchenne unfold a vast amount of information, and show a spirit of patient research which cannot be too highly appreciated.

The first part of the present volume is mainly occupied by a lengthy description of electro-medical instruments, and by an examination of the principles of localized electrization, the method with which the author's name is inseparably and honourably bound up. The second part takes up the clinical side and deals with traumatic paralysis; the atrophic paralysis of childhood, that curious and grave affection; spinal paralysis in the adult; subacute general spinal paralysis; progressive muscular atrophy and glosso-laryngeal paralysis. Duchenne has done good and lasting work in his department, to the study of which he has devoted his life, and no medical library is complete which does not possess the fruits of his energy and perseverance.

WORKS ON THERAPEUTICS.

1. *A Handbook of Therapeutics.* By SYDNEY RINGER, M.D. Second edition. London: H. K. Lewis. 1871. Pp. 483.
2. *Digitalis: its Mode of Action, and its Use.* The Hastings Prize Essay of the British Medical Association for 1870. By J. MILNER FOTHERGILL, M.D. London: H. K. Lewis. 1871. Pp. 89.

IT is about two-and-a-half years ago since the first edition of Ringer's treatise, which is founded on Buchheim's work, appeared and received its modicum of praise and dispraise at the hands of reviewers. We are glad to find that the new edition has undergone no increase in size, though some fresh matter has been interpolated, and marks of revision are apparent; and we only regret that the author has not thought fit to correct more of the faults originally pointed out. The arrangement of the contents is arbitrary and unmeaning; or rather, it is an incongruous sequence of detached articles; the index of medicines and of diseases is very imperfect, and some of his statements challenge the severest criticism. In

fine, although Dr. Ringer has had the special experience of a teacher of therapeutics, we cannot but think that there is still room for a compact and methodical English treatise on the principles of therapeutics. Meanwhile, much that is useful can be gleaned from Dr. Ringer's book, and we hope that when it becomes his duty to prepare a third edition, he will endeavour to render his work a more perfect exponent of the present state of knowledge.

Two important monographs on digitalis have been published within the past three years—one, by Dr. Brunton, noticed in this *Journal* for February, 1869, was mainly taken up with an elaborate enquiry into the physiological action of the drug; the present essay is devoted rather to the influence of digitalis, as one of a class of remedial agents, on diseased conditions of the heart. On some of the most important points they are closely in accord, and, of these, none is of higher clinical import than the doctrine, which may be held as finally settled, that digitalis acts as a direct cardiac stimulant and causes increased contraction. This increased contraction is probably due to stimulation of the cardiac ganglia, and not to paralysis of the co-ordinating vagus-fibres. Digitalis also seems to exert a direct contracting influence over the minute vessels, and this fact must not be forgotten in practice, when foxglove is administered for the sake of its effects on the muscular walls of the heart.

The long-accepted theory of digitalis being a cardiac sedative in ordinary doses must now be abandoned, and clinical phenomena certainly fit better into the modern theory. The most pressing indication for the therapeutic use of digitalis is cardiac dilatation, with or without hypertrophy; but for details as to its use in this and other special diseases of the heart, we must refer to the essay itself, and assure our readers that they will find in its pages much that is of value, and that will repay careful study.

Light Science for Leisure Hours. By R. A. PROCTOR, B.A., &c.
London: Longmans, Green, & Co. 1871. Pp. 316.

MR. PROCTOR is a contributor to the newspaper and serial press, in which capacity he frequently writes articles on scientific and semi-scientific subjects for the readers of that class of literature. This volume contains a reprint of a few of those articles. They

are written in a pleasing and popular style; but, at the same time, they are evidently the work of a writer who has a thorough knowledge of the subjects upon which he undertook to enlighten the general public. We heartily commend this volume even to those who make pretensions to be a "master in science."

The Prevention of Contagious Diseases. A Practical Treatise on Disinfectants, Antiseptics, and other Sanitary Agents. By CHARLES A. CAMERON, M.D., Professor of Hygiene in the Royal College of Surgeons. Dublin: Fannin & Co. 1871. Pp. 36.

THIS little book is a miniature treatise on sanitary science, as a large number of sanitary subjects is treated of in the briefest possible manner. The merit of the little book consists, however, in the precise directions for disinfecting rooms, clothes, &c., which it contains.

PART III.

HALF-YEARLY REPORTS.

REPORT ON

MATERIA MEDICA AND THERAPEUTICS.^a

By WALTER G. SMITH, M.D., Dubl.; Fellow and late Examiner in Materia Medica, K. & Q.C.P.I.; Assistant Physician to the Adelaide Hospital.

MATERIA MEDICA AND GENERAL THERAPEUTICS.

1. *Adulteration of Drugs.*—In some of the previous Reports the details of the examination as to purity of some of the common drugs, *e.g.*, subnitrate of bismuth, aromatic spirit of ammonia, bromide and iodide of potassium, were given, and the results, on the whole, were highly satisfactory. Mr. Allen has lately examined several specimens of *permanganate of potassium*, obtained in different parts of England, and found them to contain from 93 to 98 % of real permanganate, so that they were practically pure. Traces of sulphate, but no chlorides, were present, and a little free manganese dioxide. The oxidizing power of Condry's crimson fluid is about 1.7 times that of the liq. pot. permang. (B.P.) Condry's green fluid, containing chiefly manganate, is nearly equal to the crimson fluid.—(*Pharm. Journ.*, Sept. 2, 1871.)

For the detection of turmeric in powdered *rhubarb* and *yellow mustard*, Prof. Maisch gives the following process:—Agitate with strong alcohol, filter, and add a strong solution of borax. If now pure muriatic acid be added in large excess, the unadulterated tincture will become lighter in colour or whitish, while if turmeric be present a brown-red colour remains due to the action of the free boracic acid on the curcumin of the turmeric.—(*Pharm. Journ.*,

^a The author of this Report, desirous that no contribution to the subjects of Materia Medica and Therapeutics should remain unnoticed, will be glad to receive any publications which treat of them. If sent to the correspondents of the Journal they will be forwarded.

June 24, 1871, from *Amer. Journ. of Pharm.*) This boracic acid test is utilized in the B. P., under "Rhei Radix."

Adulteration is carried to a gigantic extent in America, for, in some establishments, there is a regularly organized adulterating department, with a foreman to superintend this special branch. All sorts of cheap and refuse substances are available for this branch of industry, and some dealers, it appears, uniformly send to the grinders a certain proportion of liquorice with the opium, so that they may be ground together. Mr. Remmington, of Philadelphia, quotes numerous glaring examples of fraud, especially in the case of powders, which are the most liable to adulteration, and his report to the Pharmaceutical Conference at St. Louis exposes some startling revelations of the tricks of the trade.—(*Brit. Med. Journ.*, Nov. 18, 1871.)

Commercial *Iodide of Potassium*, procured in London, has been found to be remarkably pure, but now, in consequence of the extraordinary rise in its value, there will be every temptation to adulterate it. In June last it was 16s. 6d. per lb.; in Aug., 24s. 6d.; in Oct., 30s. 0d.; in November 38s. 0d.; and its price has since gone up even higher. The probable causes of this sudden elevation of price are—(1) Increased consumption of the salt in medicine; (2) Demand for its use in photographic processes; (3) Iodine, in some form or other, is said to be used largely as a mordant in the manufacture of some green dyes much in request.—(*Lancet*, Dec. 2, 1871.)

Dr. Burgemeister gives the following as tests for pure *glycerine*. Mixed with an equal volume of pure oil of vitriol it remains clear and colourless, or nearly so. Impure glycerine gives off gases (CO_2 & CO) and oxalic and formic acids are also formed. Gum would be indicated by the formation of turbidity or of a gelatinous precipitate on the addition of rectified spirit. Heated with caustic potash it should not develop a brown colour, nor the odour of ammonia.—(*Med. Times and Gaz.*, Oct. 21.)

2. *Chemistry of effacing Iron-moulds and Manganese-stains.*—Mr. J. Brown on testing the comparative efficiency of three familiar solvents of ferric oxide, viz., hydrochloric acid, oxalic acid, and quadroxalate of potassium (sal acetos), found that the latter is superior in solvent power, and is by far the best agent for removing either iron-moulds or the disfiguring brown stains produced by Condyl's fluids. The reaction of peroxide of iron with sal acetos is:— $\text{Fe}_2\text{O}_3 + 2 (\text{KHC}_2\text{O}_4\text{H}_2\text{C}_2\text{O}_4) = 2\text{FeC}_2\text{O}_4 + \text{K}_2\text{C}_2\text{O}_4 +$

$2\text{CO}_2 + 3\text{H}_2\text{O}$. With black oxide of manganese, the equation is:— $\text{MnO}_2 + 2 (\text{KHC}_2\text{O}_4\text{H}_2\text{C}_2\text{O}_4) = \text{MnC}_2\text{O}_4 + 2\text{KHC}_2\text{O}_4 + 2\text{CO}_2 + 2\text{H}_2\text{O}$.—(*Pharm. Journ.*, Sept. 30.)

3. *Koumiss*.—In the steppes of Russia, the hardy and robust people live chiefly on Koumiss, a piquant, fragrant liquor, prepared from mare's milk by fermentation. The remarkable exemption from phthisis enjoyed by these people is ascribed by some to the use of Koumiss. Dr. Jagielsky has succeeded in producing from cow's milk a Koumiss, the counterpart of that from mare's milk, and now introduces it to the profession as a valuable dietetic remedy in complaints arising from feeble digestion; in chronic diarrhœa; the adynamic stage of fevers; and in bronchitis and consumption. Koumiss is used in various forms, as thick Koumiss, whey Koumiss, &c., and of different degrees of age, and may be pushed freely to the extent of two quarts per day.—(*New Remedies*, July, 1871, from *Food Journ.*)

4. *Senna*.—MM. Bourgoïn and Bouchut conclude from their investigations that no single one of the constituent principles of the plant, taken by itself, can pretend to represent the whole of its general properties. Besides chrysophanic acid, which only exists in small quantity, senna contains at least two other purgative principles: one represented by cathartic acid, the other contained in the preparation improperly called cathartin. The best preparation of senna is the infusion, with or without the mucilaginous matter.—(*Pharm. Journ.*, Sept. 16, from *Journ. Pharm. et de Chim.*) Mr. Groves had already pointed out, in 1868, that senna should invariably be exhibited in a watery vehicle, as strong spirit fails altogether to extract its active principle.

5. *Iodide of Potassium*.—*Pemphigus from*.—The following case occurred in Dr. Bumstead's practice. A man, aged 28, was ordered 20 gr. of the iodide twice a-day for syphilitic ulcerations on the legs. Next day, after having taken only three doses, he complained of a burning sensation in the face and hands, which were observed to be reddened, and the medicine was at once discontinued. On the following day small purpuric spots were thickly out on the feet and legs, and, in addition, there were large bullæ, some $1\frac{1}{2}$ inches in diameter, situated upon those portions of the skin which were exposed to the air. In a few days most of

them had ruptured and were in process of drying up. On three previous occasions the patient had taken iodide of potassium, and always with the same unpleasant results. This result is extremely rare, but Boinet states that Cazenave had seen this salt cause a bullous eruption.—(*Amer. Journ. of Med. Sci.*, July, 1871.) In the last Report (Art. 28), a case was quoted in which *purpura* twice followed the administration of iodide of potassium; an effect previously observed by Ricord and Virchow.

5. *Bromide of Potassium*—(α). *Ill effects of*.—Attention has of late years been repeatedly drawn to the inconvenient consequences occasionally attending the use of bromide of potassium, especially when pushed freely for some time. Several additional instances have been published during the last few months, and it is manifestly of importance to keep these facts in mind. Dr. Noble has seen general depression and a form of muscular ataxia supervene on the administration of 3ss. doses for some weeks to an epileptic patient. Dr. Needham, though believing that untoward symptoms are rarely manifested, has twice seen scruple doses, given three times a-day, produce within a week extreme depression, rapid wasting, impairment of muscular power, dilatation of pupils, hesitation of speech, and great taciturnity, together with loss of mental power. Mr. Provis has met with somewhat similar symptoms, attended with impairment of memory, and an ecthymatous eruption. Dr. Foss' experience has been similar, and in several instances he has also observed symptoms of gastric disturbance, as indicated by loss of appetite, pain after eating, vomiting, and epigastric tenderness.—(*Brit. Med. Journ.*, Sept. 23.)

Mr. T. O. Wood, who has had considerable experience of this drug in the treatment of the insane, remarks that, when given continuously and in large doses, it produces a great variety of results, depending generally upon the constitution and bodily condition of the patient at the time of its administration. Its most dangerous consequence, however, is when, after a course of comparatively small doses, it *suddenly* displays its cumulative effects, rapidly reduces the patient to a condition of great bodily prostration, and completely alters the character of the mental symptoms. The symptoms are—great muscular debility; dimness of sight, with dilated pupils; irregular, reeling gait; while nausea, vomiting, or purgation, with dull abdominal pain, may also be present. The breath has a disagreeable odour, which seems peculiar to those who have been for any

length of time under the influence of the bromide. Its effects on the mental condition are no less marked. The patient, who has been violently excited, glorying in his imaginary power of body and mind, becomes desponding, sullen, melancholic, and frequently lachrymose, often even despairing. The combination of cannabis indica with the bromide, as suggested by Dr. Clouston, seems to prevent the injurious cumulative effects of the latter.—(*Brit. Med. Journ.*, Oct. 14.)

(β). *Corrective influence on Opium*.—In cases where opium cannot be borne alone, Dr. Da Costa states that he has had great success by the conjoint use of it and bromide of potassium. He gives two or three 20 gr. doses—one, half-an-hour before the narcotic, the other, three hours afterwards, or sometimes a larger dose with the opium. The faintness from opium is the symptom most markedly prevented, next, the headache, vertigo and nausea — (*Amer. Journ. Med. Sci.*, April.)

7. *Nitrite of Amyl*.—Mr. A. Tanner prepares this compound ether, discovered by Balard in 1844, by the following process, analogous to that for procuring sp. ætheris nitrosi. To the *purified* amylic alcohol (fousel oil) introduced into a glass retort, containing some copper wire, is added $\frac{1}{10}$ of its bulk of sulphuric acid. The same quantity of nitric acid diluted with an equal volume of water is next added, and a gentle heat applied at first so as to start the distillation. More nitric acid is gradually supplied until nearly the whole of the amylic alcohol is exhausted, which may be known by the dense red fumes evolved from the retort. The distilled product, when washed with solution of soda, and rectified over fused carbonate of potassium, is sufficiently pure for medicinal use.

Mr. Umney has shown that much of the amyl nitrite as sold is very impure, and it is liable to contain not inconsiderable quantities of hydrocyanic acid. Other impurities are ethyl-amylic ether and amylic aldehyd; and the presence of these impurities probably accounts for the unpleasant symptoms sometimes observed to follow its use. When pure and perfectly dry, amyl nitrite boils at 99°C. Its sp. gr. is 0.877; it is almost insoluble in water, but freely so in ordinary alcohol, ether, chloroform and benzole, and it is itself a solvent of fats and oils. Unlike nitrite of ethyl it does not appear to suffer decomposition by keeping.—(*Pharm. Journ.*, Nov. 25.)

In the *Practitioner* for October, Dr. Talfourd Jones contributes

an interesting account of his experience of this compound. He has administered it to about fifty friends and patients, and has found that its inhalation invariably causes increased frequency of the cardiac pulsations, accompanied by flushing of the face and neck and perspiration. It sometimes causes a little breathlessness, or now and then giddiness and a feeling of intoxication. As a remedy he has employed it by inhalation, 5 drops on a small piece of lint, in the following diseases, and has never seen any bad effects from its use. In two cases of *spasmodic asthma* the most immediate and striking relief followed its inhalation, and a comfortable night was obtained. In *cardiac dyspnœa*, *angina pectoris*, and *syncope* it also proved most beneficial, and in the case of angina the patient had used many ounces of the fluid with invariable relief and without any bad results. He has also used it in a case of *laryngeal spasm* in phthisis, in *intestinal colic* and in *hemicrania* ("sun-pain") with good effect. In the same Journal a case is quoted from Dr. Oskar Berger, of Breslau, of severe spasmodic hemicrania in a lady, aged 24, in which immediate banishment of the pain was obtained by the inhalation of 5 drops of the nitrite.

8. *Chloroform Accidents.*—A controversy recently arose between Mr. Lister and Mr. Clover respecting the safest method of administering chloroform, and the best means of averting threatened danger, points which ought to admit of a satisfactory answer. Mr. Clover has given chloroform more than 7,000 times, and has never lost a patient from it or any other anæsthetic. Mr. Lister recommends a folded towel as at once the simplest and safest means of administering chloroform, and holds that when this is used the respiration becomes obstructed before the circulation fails, and that circumstances occasionally arise in which the patient's life will be placed in imminent jeopardy unless the obstruction be cleared away by firm traction upon the tongue; and further that, as this interference with the breathing may occur in a very insidious manner, it is wiser for the administrator to keep his attention riveted upon the respiration rather than have it distracted by simultaneous observation of the pulse. Mr. Clover, on the other hand, regards the folded towel as a dangerous means of administration in comparison with an apparatus of his own devising, which has proved so successful in his hands, and he has never seen it needful to draw forward the tongue except in so far as that is done by raising the chin. But he thinks it necessary to observe the pulse, and he believes that if he had

neglected it, his practice would not have been free from fatal event.

Mr. Lister estimates the cases in which he has been concerned in the administration of chloroform as about 3,500, and since in these cases the chloroform was, as a general rule, not given by himself at all, but by young men coming fresh to the duty every three months, who were instructed in Mr. Lister's method, the freedom of his experience from fatal accident is far greater evidence of the safety of the cloth than Mr. Clover can afford respecting his apparatus. Mr. Clover is convinced that the chief cause of danger is the effect of chloroform on the heart, while Mr. Lister believes that the great majority of the alleged instances of death from chloroform by syncope may be accounted for in other ways. Mr. Clover's apparatus for regulating the percentage of chloroform vapour in the air inspired is certainly cumbrous, and perhaps Mr. Lister is not far astray in regarding it at best as a harmless luxury. Mr. Lister but follows the practice of the late Mr. Syme in the administration of chloroform, who had given this anæsthetic upwards of 7,500 times without a death occurring, and though he omits to explain the deaths which have occurred both in Edinburgh and elsewhere, where the tongue has been drawn forward and artificial respiration set up, he has taken up and maintained a strong position which it will not be easy to gainsay.—(*Brit. Med. Journ.*, July 8; July 29; Sept. 2.)

9. *Absorption of Medicines through the Skin.*—(α) *From Baths*—In a paper in the *Wiener Med. Wochenschrift*, Dr. Chrzonzewski relates experiments upon this subject, made both upon animals and man. The anus and urethra were covered up, and in thick-haired animals the skin was shaved. The result was that the animal died in 18 or 20 hours in a 2 per cent. solution of muriate of morphia; in $2\frac{1}{2}$ hours, in 1 per cent. of strychnia; in 1 or $1\frac{1}{2}$ hours, in 1 per cent. of nicotia; in 2 per cent. of cyanide of potassium at 2°C in $\frac{1}{2}$ or $\frac{1}{3}$ hour; more quickly at a higher temperature. A boy, aged 15 years, remained 6 hours in a sitz-bath (65°C) of infusion of digitalis ($\frac{1}{2}$ lb. to 4 buckets of water); 14 hours afterwards the pulse fell from 84 to 60, gastric and cerebral symptoms came on and lasted two days. One of the experiments instituted to test the method of absorption was—Ferrocyanide of potassium was injected into the vein of a dog, and the animal placed in a bath, containing an iron salt; in 3 to 5 hours the veins and capillaries of the skin

were stained to an intense blue, the cellular tissue remaining colourless. The following conclusions were arrived at:—1. The skin of man and animals is permeable to substances in watery, and still more easily in spirituous solutions. 2. Elevated temperature hastens absorption. 3. Absorption may take place in various ways, diffusive (indigo carmine); through the blood-vessels (formation of Prussian blue); and through the lymphatics (ammoniacal solution of carmine). 4. The lymph vessels have their origin in the cells of the connective tissue.—(*New Remedies*, Oct.)

(β). *Of Mercury*.—Dr. Neumann finds as the result of many experiments, microscopical and otherwise, that in the inunction of grey ointment on an unbroken skin, mercury-globules pass into the hair-sheath, then into the bulb, and also into the superficially opening sebaceous glands, and into upper part of sweat-glands. In what way and what form they get thence into the circulation he could not discover; probably as corrosive sublimate. In the blood and internal organs, mercury, which has been introduced by inunction or by sublimate baths, can only be detected by chemical means. Mercury could not be detected by chemical means in the subcutaneous tissue, and does not penetrate the horny epidermis.—(*Practitioner*, Nov.)

(γ). *Poisoning by Absorption of Corrosive Sublimate*.—An inquest was held at Bath, last autumn, on a child of Mr. Fowler, M.P., aged nine years, who died from the absorption of perchloride of mercury, applied once locally in the well-known caustic solution of 10 grains to the drachm for the cure of ringworm. The application is one which has long been most extensively used for the destruction of nævi, and as a topical parasiticide. It is recommended in the text-books, and has been used innocuously and with great benefit in probably many thousand cases. There appears in this case to have been an unusual susceptibility, for the substance was absorbed, and the poor child died with symptoms of mercurial poisoning; and the jury attributed—most unjustly—blame to the surgeon in their verdict.—(*Brit. Med. Journ.*, Sept. 16.)

10. *Bichloride of Methylene*.—Mr. F. Searle adds his testimony in favour of the above. In 1870 he administered it to more than 100 cases in the West of England Eye Infirmary, and has since used it in private practice. He has given it at all ages from 6 months to 70 years, without a single cause for alarm, vomiting even being exceptional. It is important not to allow the patient to escape from

its first influence, otherwise excitement ensues.—(*Lancet*, May 27.) In Padua, bichloride of methylene has been employed for three years in the surgical clinic to the exclusion of ether and chloroform, being sent over from London by Messrs. Robbin. Out of 108 operations performed under its influence, Dr. Rossi states that 52 patients slept tranquilly without any muscular agitation within from 1 to 5 minutes; 32 experienced slight excitement and became insensible in 8 or 10 minutes. Four only were violently agitated, and sleep was not induced for 15 or 20 minutes; 20 remained completely free from its influence even after 50 minutes of inhalation. Vomiting occurred in 8 instances. No other accident declared itself, and the liquid excited no cough, but slight lachrymation. The pulse and respiration were increased in frequency from the first, but soon returned to the normal state, and even fell below it. The face underwent no change of colour.—(*Journ. de Pharm. et de Chim.*, Sept.) These results correspond in many particulars with those previously attained in England by Mr. Miall and Mr. Gaine, and this anæsthetic seems well worthy of future trial.—(See Reports, Aug., 1870, and Aug., 1871.)

To the three *deaths* already recorded as happening under its use must now be added another from the Radcliffe Infirmary, Oxford. Bichloride of methylene was administered on a flannel bag to a woman, aged 44, who was about to undergo an operation for cancer of the breast. After two or three convulsive gasps the patient died, though the quantity administered was small.—(*Brit. Med. Journ.*, Sept. 16.)

11. *Organic Bromides*.—Dr. B. W. Richardson proposes for use the bromides (hydrobromates?) of quinia, morphia, and strychnia. They may be given in syrupⁿ in doses, respectively, of 1 to 4 grs.; $\frac{1}{8}$ to $\frac{1}{2}$ gr.; and $\frac{1}{32}$ to $\frac{1}{8}$ gr. He believes that the general effect of the addition of the bromine is eliminative and sedative, and he is satisfied that bromide of quinia can be freely administered when other salts of quinia cannot be borne, that the bromine favours the sedative action of morphia, while it at the same time allays the astringency which morphia induces, and lastly, that bromine reduces, or rather subdues and prolongs the action of strychnia on muscular motion. Bromide of quinia is of good service in certain special and persistent symptoms which follow upon syphilis, *e.g.*, relapsing rheumatism, recurring ulceration of the fauces, &c. Bromide of morphia is especially useful when combined with

bromide of quinia, and acted most favourably in acute neuralgia, diabetic phthisis, and cerebral irritation. Bromide of strychnia is decidedly useful in cases of dyspepsia with deficient nervous control in the digestive organs. A syrup of the three bromides will probably become a favourite combination. The bromides are contraindicated in cases of dryness and irritability of the pharynx and larynx.

Bromide of Ethyl ($C_2H_5Br.$), a light volatile liquid, was proposed by Mr. Nunneley as a safe general anæsthetic, but Dr. Richardson cannot recommend it on account of its liability to cause irritation of the throat and vomiting, and because the fluid easily decomposes and liberates Br.

Bromide of Methyl ($CH_3Br.$), a gas at ordinary temperatures, resembles bromide of ethyl in its properties. Both are powerful deodorizers and destructives of decomposing organic matters.—(*Practitioner*, June.)

12. *On the Principles of Therapeutics.*—Dr. James Ross, in continuation of his remarks on this subject (see Report, Aug., 1871), contributes an interesting paper to the *Practitioner* for Aug., 1871. The healthy body is in more or less stable equilibrium, though an equilibrium of oscillation, for the body does not maintain the same equilibrium for two successive instants of time. In fact, looking at the body as a whole, the path of its healthy life may be compared to a parabola. But, though this is generally true, since each part of the body has a life of its own, it is upon the orderly progress of the life of each organ that the harmonious action of the whole depends. In disease the perturbations ought to be regarded from the two-fold point of view—of the life of the individual as a whole, and the life of the different parts or organs of the individual. The action of many medicines may be referred to that law of equilibration, which means that in which the primary oscillation is succeeded by a secondary one in an opposite direction until, finally, the equilibrium is either overthrown in death, or the medium condition restored.

In the treatment of disease the typical plan is to endeavour to deflect the course of the diseased organ to the standard health of the individual, or, when that is impossible, the next best thing to do is to enable the whole body to form a new equilibrium about the diseased organ, *e.g.*, in the case of fatty heart, &c. These two principles are applicable to the treatment of almost every case of disease.

SPECIAL THERAPEUTICS.

13. *Phosphorus*.—(α). in *Diseases of the Skin*.—Dr. Broadbent read before the Clinical Society of London, last year, a paper on phosphorus as a remedy in skin diseases, in continuation of a series of cases published in Vol. ii. of the Society's Transactions, in which manganese and nickel were given as remedies for anæmia. If the action of remedies and poisons on the human organism be due to their chemical properties, substances allied chemically ought to have an analogous physiological and therapeutical influence, or the diversity in their action ought to be explicable on chemical grounds. In other words, chemical groups should form therapeutical groups. The four members of the group of which phosphorus is the head chemically, and of which arsenic is the chief representative in therapeutics—phosphorus, arsenic, antimony and bismuth—stand in the order named in regard to equivalent numbers, physical properties and chemical energy. Excluding bismuth, which is comparatively inert, there is in the mode of action of phosphorus, arsenic and antimony, as poisons, and in the tissue changes which they induce, a parallelism as remarkable as that of the chemical properties of these bodies, both in the energy and in the character of the physiological effects. The opportunity for bringing out further therapeutic parallelism is furnished by the well-known curative action of arsenic in certain classes of skin-disease, such as some forms of eczema and psoriasis. In cases of this kind, instead of arsenic, phosphorus was given. Two grains of this substance were dissolved in oil, and from 3 to 7 drops of the solution were given, usually in mucilage, three times a-day after meals. Six cases of eczema were related, in all but one of which the phosphorus was decidedly beneficial. The cases of psoriasis were also six in number, two of which proved rebellious, not only to phosphorus, however, but to arsenic, and to all treatment, general and local. If phosphorus were as manageable and as little disagreeable as arsenic, it would probably be found superior in efficiency.—(*Brit. Med. Journ.*, May 6.)

In the January number of this Journal, Dr. Eames has published an interesting paper, to which we would recall attention, tending to prove that the administration of phosphorus will cure certain cases of cutaneous disease, even after mercury, arsenic, and other so-called specifics have completely failed to do so. The cases

included one of severe *acne indurata* of the face, of four years standing, in which a cure was effected in six weeks; three of lupus, in one of which phosphorated oil was taken for nine months; two of scrofuloderma; three of psoriasis; and one of pemphigus. Dyspepsia is of not infrequent occurrence, but is readily met by substitution of mineral acids for a short time.

(β). *In Cataract.*—Dr. Tavignot urges the value of phosphorus, both locally and generally, in the treatment of cataract, and in the *Revue de Thér.*, No. 20, 1871, cites a number of cases from Dr. Alken's practice, showing the efficiency of this method of treatment.

14. *Perchloride of Iron as a prophylactic of Acute Rheumatism.*—Influenced by Dr. Russell Reynold's observations on the treatment of acute rheumatism by large and frequent doses of perchloride of iron, Dr. Anstie has of late been employing it in the treatment of the prodromata of acute rheumatism, as seen in out-patient practice, viz.: aching of joints, sallowness of face, with patches of dusky redness, blanket-like furring of the tongue, oily moisture of the skin, slight elevation of pulse and temperature, and some anxiety of respiration. He believes that by giving 30 or 40 minims of the Tinct. Ferri Perchlor. three to six times a day, the great majority of cases can be prevented from developing into the fully declared disease.—(*Practitioner*, Sept., 1871.)

15. *Stimulating Hypodermic Injections.*—Some German journals have recently reported several interesting observations on the therapeutic value of stimulating hypodermic injections in different diseases of an asthenic type, and more especially in the typhoid fever, which prevailed in the Prussian army during the last campaign in France. Dr. Zuelzer has used with much advantage a new curative method in these maladies, viz.: an hypodermic injection of 6 or 8 drops of alcohol or Liq. Ammonia. By this method Dr. Zuelzer ascertained that the pulse from being small and irregular became quickly full and strong, that the cardiac contractions, at first weak and feeble, became regular, energetic, and visible to the eye, and, in fine, that the cyanosis and collapse quite disappeared. The small abscesses which sometimes form in consequence of the irritating nature of the injection are, it is stated, of no importance, as in the greater number of cases they are spontaneously resolved.*—(*Lo Sperimentale*, July.)

* I am indebted to Dr. G. F. Duffey for the translation of this, as well as of some other articles, not included in the Report.—W. G. S.

16. *Chromic Acid*.—Dr. J. Dougall supports warmly the value of chromic acid (anhydride) as an antiseptic, disinfectant, and germ-preventive. Its coagulating power is about 10 times that of carbolic acid, 15 times that of nitric acid, 20 times that of corrosive sublimate, and 150 times that of chloralum. Its reaction with gelatine is as delicate as that of tannic acid, giving a response with 1 in 5,000. Chromic acid is well adapted for estimating albumen volumetrically, thus:—Fill a wide-mouthed burette to a multiple of 100 with the albuminous liquid (say, urine), add solution of chromic acid (4 grs. to 3i) in slight excess; shake, set aside for 24 hours and read off. Carbolic acid does not combine with ammonia or sulphuretted hydrogen; chromic acid does.—(*Lancet*, Dec. 16.)

17. *Electricity*—(*α*). *in Constipation*.—A lady, aged 24, subject to repeated attacks of rheumatism and of constipation, was affected with a fresh access of the latter, against which all remedies were employed in vain. On the fortieth day the symptoms being very alarming, and death impending, the negative pole of Gaiffe's induction apparatus was introduced into the rectum, the positive being placed over the umbilicus. A feeble current was at first passed, but its strength was steadily increased and maintained for 20 minutes. Ten minutes after the application was discontinued there was a free passage from the bowels, and all the general symptoms were forthwith relieved.—(*New Remedies*, Oct., from *Bull. Gén. de Thér.*, Juin 15.) Dr. Macario published in the *Annali. Univ.*, for Oct., 1870, a case of *ileus* in a gentleman, aged 71, in which a strong current passed for ten minutes proved successful.

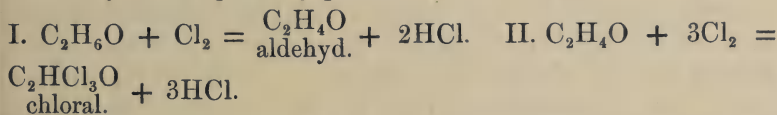
(*β*). *In Paralysis of Arm*.—Dr. Althaus had lately under his care a girl, aged 16, who presented in a marked degree the curious symptom of insensibility to faradization, both muscular and cutaneous, and perfect excitability to galvanization. The right arm, without any apparent reason, was gradually attacked by complete motor and sensory paralysis, with great depression of the circulation. She was treated at one hospital for nearly four months, chiefly by the induced current, without any benefit. Faradization of the skin, nerves, or muscles of the arm was not in the least perceived, but 60 Daniells' cells caused sensations of tingling and heat in the arm, and sluggish contractions of the muscles. Within 4 days common sensation had completely returned, and after 10 days the first feeble motion appeared in the fingers. The improvement became sta-

stationary after a time, but in time she may completely recover.—(*Brit. Med. Journ.*, Nov. 5, 1870.)

(γ). *In Paralysis of the Bladder*.—Dr. Althaus, eliminating from this affection all cases of mere atony of the viscus from over-distension, as well as all cases of incontinence of urine due to paralysis of the sphincter of the bladder, expresses the opinion that, both in efficiency and quickness of action, the constant (direct) galvanic current, properly applied, is superior to all other remedies used for this affection. In three cases, one owing to syphilitic disease of the cerebral peduncle; another in a hysterical patient; and the third due to disease of the lumbar cord, the bladder recovered rapidly under the influence of the constant galvanic current.—(*Brit. Med. Journ.*, Aug. 26.)

CHLORAL.

Preparation and Properties.—Mr. W. A. Tilden, D. Sc., has published in the *Pharm. Journ.*, for Oct. 28, and Nov. 18, an elaborate and interesting paper on chloral, containing much that is of practical interest. In the preparation of chloral by the action of *dry* chlorine on *absolute* alcohol, it is necessary to exclude water from the materials employed, because chlorine in presence of water acts upon organic bodies, not by substituting itself for their hydrogen, but by oxidizing them. Consequently, when water is present, the aldehyd and acetic acid that are usually unavoidably produced in small quantity, become the chief results of the reaction. It appears that alcoholate of chloral is the immediate offspring of the action of the chlorine, and that from this the chloral is set free by the sulphuric acid afterwards used. Water is, however, certainly formed in small quantity by a secondary action, and therefore a little hydrate is probably present as well. The reactions are:—



Chloral, however, is not by any means the only resultant of the reaction, for subordinate and complicated interchanges go on at the same time, which result mainly from the influence of the hydrochloric acid generated, and of the chlorine on the substances produced, and on the impurities in the spirit. From these causes we get as concomitants of the chloral, chloride of ethyl, crotonic chloral, acetic acid, and ether (?), and the numerous compounds derived from the

fousel oils, impurities in the spirit. In addition, most of these various substances are susceptible in their turn of the substituting action of chlorine, yielding a long list of by-products.

Chemical Characters.—In addition to the properties mentioned in former Reports, some further characters may be noticed. It melts at 122° — 123.8° F. (50° to 51° C.), and boils at a temperature a little below 212° F. The hydrate of chloral contains 89.12 per cent. of chloral, and yields, with an alkali, 72.2 per cent. of chloroform. Alcoholate of chloral contains 76.22 per cent. of chloral, and yields 61.76 per cent. of chloroform. Chloral and its aqueous solution are said to dissolve many of the alkaloids, *e.g.*, morphia, quinia, and strychnia. It can also take up considerable quantities of some substances, such as camphor, carbolic and benzoic acids, which are by themselves but sparingly soluble in water. Chloral hydrate is miscible with glycerine. Chloral appears to be purely an aldehyd; in fact, the aldehyd of trichloroacetic acid, and it resembles ordinary acetic aldehyd in its general reactions, though, on account of the presence of chlorine, differing from it in some important particulars. Sulphuric acid mixed with hydrate of chloral produces considerable depression of temperature, dehydrates it, and on gently heating, liquid chloral separates, which after a time becomes partially opaque from the formation of metachloral. If a considerable excess of sulphuric acid be employed, there is produced another solid body, *chloralide* ($C_5H_2Cl_6O_3$). Fixed alkalies decompose chloral into formic acid and chloroform; ammoniacal gas unites with chloral, if kept perfectly cold, to form $C_2HCl_3O.NH_3$, the analogue of aldehyd ammonia; but if allowed to become heated, chloroform and formamide ($CHO.H_2N$) are produced. Chloral unites with many of the derivatives of ammonia, *viz.*, amides and amines. Nascent hydrogen re-converts chloral into aldehyd, and oxidizing agents, chromic or nitric acid, convert it into trichloroacetic acid:— $C_2HCl_3O + O = HCl_2C_3O_2$. Nitric acid of sp. gr. 1.2 appears to have but little, if any, action upon chloral hydrate. Lastly, the quantitative tests as yet recommended (see Report, Feb., 1871), although possessing a certain value, do little towards the detection of those other chlorinated bodies which may so easily occur, though only in small quantities, without suspicion, and it has not been proved that the liquid which separates in the reaction with alkalies is really pure chloroform, or whether it may not also include products of the decomposition of those other bodies.

Physiological Action.—In a note presented to the *Academie des*

Sciences, M. Byasson formulates some conclusions, which differ from those of Dr. Oscar Liebreich, and have been founded upon the comparative action of chloroform, formate of sodium, hydrate of chloral, trichloroacetic acid, and trichloroacetate of sodium upon frogs, rats, and dogs—and incidentally of hydrate of chloral upon man:—

1. The action of chloral is different from that of chloroform.
2. This action is peculiar, but may be looked on as the result of the conjoined action of chloroform and formic acid.
3. The action of chloral differs from that of trichloroacetic acid, which breaks up into chloroform and acetic acid.
4. Part of the chloroform formed is eliminated by the lungs; and part of the formic acid is eliminated in the urine as formate of sodium. There are three degrees of the operation of chloral on the animal.—(a). A feebly soporific and slight nervous sedative action. (b). An intense soporific action, with diminution of sensibility. At this period there succeeds calm slumber of variable duration, without apparent trouble of the principal functions of life. (c). Complete anæsthesia and loss of muscular power, generally followed by death.—(*Pharm Journ.*, Dec. 16.)

Eruption from use of Chloral.—Dr. Husband gave 20 grs. of chloral, in place of morphia, to a patient suffering from organic disease of the uterus. A few days afterwards, the dose was increased to 30 grs., and continued for four days, when she became covered from head to foot by a red eruption, accompanied by fever and irritation of the skin. This lasted for several days, and then the skin desquamated similarly as in scarlet fever.—(*Lancet*, June 24.) Dr. Fisher had previously noticed the occurrence of urticaria from its use.

Consumption of Chloral.—Chloral is literally being used by the ton, and Baron Liebig was told by a chemical manufacturer in Germany that he makes weekly *half-a-ton* of the hydrate. It is not easy to account for this enormous rate of consumption, far beyond what can be demanded in the sphere of medicine alone, though it has been stated that some of the chloral finds its way into beer, and that chloral-dosing is the new and popular vice, particularly among women. There is now a duty on chloral of 1s. 3d. per lb.

Toxical Effects.—Several examples of the power of chloral for harm were adduced in the Reports for February and August, 1871, and others have been since reported. Dr. N. R. Smith, of Baltimore, has observed in two cases, during the continued and liberal use of

chloral, a singular affection of the fingers, attended with desquamation and superficial ulceration, especially about the borders of the nails.

He also states that he is acquainted with two similar cases. He mentions besides three cases in which death was caused by, or at least followed on, the administration of large doses of chloral.— (*Lancet*, Sept. 30.)

Dr. Andrews, of New York, has published a short essay on the physiological action and therapeutic use of chloral, which confirms, generally, the results of other observers, especially as to its use in asylum practice.

PART IV.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

Wednesday, January 17th, 1872.

DR. HUDSON, President, in the Chair.

DR. HAYDEN read the following paper, descriptive of a stethometer, which he exhibited:—

A stethometer should be designed to determine, not only the absolute and relative girth, but likewise the absolute and relative expansibility of the two sides of the chest.

The measurement of one or both sides must be taken with reference to the status of respiration at the moment selected for the experiment.

Thus, in the state of complete contraction of the healthy thorax, as after a forced expiration, when the lungs contain only the *residual* air, to use the language of Hutchinson; in that of partial contraction after an ordinary expiration, when the additional volume of *reserve* air is present; in the state of partial expansion attained by an ordinary inspiration, by which the volume of *tidal* air is superadded; and, finally, in the state of complete expansion of the chest produced by a forced inspiration, in which the volume of air known as *complemental* is likewise introduced, the absolute diameter of both sides of the chest must vary within the range of health; and in one or more of these states the relative diameter of either side may likewise differ. When the measurement of the chest, whether absolute or relative, is recorded, it is therefore necessary to mention under what state of respiration it has been made; otherwise errors which must vitiate the result may be admitted.

For example, a lung in the early stage of diffused tuberculosis may give to the corresponding side of the chest a diameter identical with that of the opposite and sound, or less affected side, in the state of forced, or even in that of ordinary expiration; whereas in the state of partial

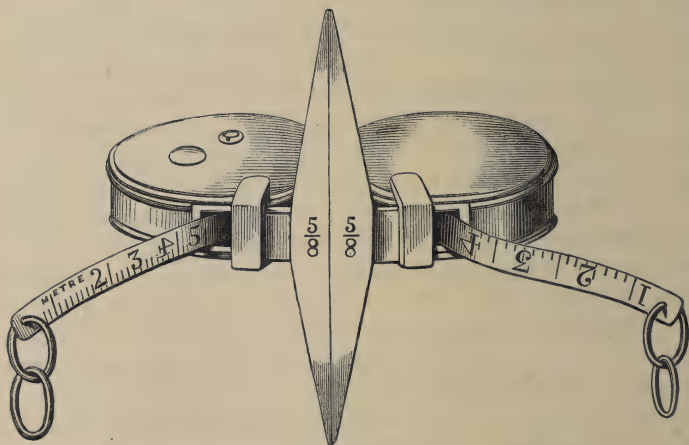
expansion of the chest produced by an ordinary inspiration, the two sides may be found to differ very considerably.

A case is conceivable likewise, in which, under similar circumstances, or where one lung is adherent, or invested by false membrane, although no difference of diameter between the two sides of the chest may exist after an ordinary inspiration, a considerable disproportion may be exhibited by them in the state of full inspiration. Conversely, in the state of maximum inflation of the chest the two sides may be strictly equal, whereas, in all states of minor voluntary expansion a notable inequality may be exhibited, as for example in pulmonary emphysema. The disproportion in these several instances, and in many others which might be mentioned, must, in the usual method of procedure, be determined by repeated measurement of the chest in the different states of respiration indicated.

A succession of experiments must involve complex calculations; and irrespectively of sacrifice of time, is not unlikely to yield erroneous results.

It is desirable, therefore, for the purpose of saving time, and likewise as a means of attaining greater accuracy in the result, to determine by a single operation, not only the measurement, but also the *expansibility* of the two sides of the chest, absolutely and relatively to one another.

The small instrument which I now exhibit to the Society has been designed to accomplish this two-fold object. I have been, for more than a year, making actual use of it in hospital; and although I do not recommend it as perfect, I believe it to be of considerable utility in practice.



The Stethometer has been made, after Dr. Hayden's design, by Mr. Whyte, cutler, 68, Upper Sackville-street.

It consists, as will be observed, of two attached box-cylinders, around each of which a graduated steel tape is wound by a spring, put in action by pressure with the thumb-nail upon a small nut on the flat surface of the corresponding cylinder. Intermediate between the cylinders, and fixed to the edges of both by a cross-plate, is an indicator, divided in its length on the superficial surface into two equal parts by a fine line, the distance from which on each side to the point of emergence of the steel tape from the interior of the cylinder, is five-eighths of an inch, and in calculation of the absolute girth of each side of the chest, this distance is to be added to the length of tape actually drawn out of the cylinder. Passed through an aperture in the free extremity of the tape is a small steel ring, to which, for convenience of prehension, another ring of the same size is attached. An ink-mark is made exactly in the middle line of the spine and of the sternum respectively, and in the same horizontal plane, one inch above or below the level at which it is proposed to take the circumference of the chest; the corresponding point of the indicator is applied to the anterior of these marks, whilst by its length it extends along the mesian line upwards or downwards, as the case may be, and in that position it is held by the patient or an assistant.

The tapes are now drawn out of the cylinders by means of the rings, and carried around the chest till they meet at the second ink-mark on the posterior surface of the body, the subject of the experiment having been, meantime, directed to contract or expand the chest to the state in which it is desired to register its measurement. The scale on the tapes is now read off, and the absolute girth of each side at once determined; whilst by comparison the relative proportion may be likewise ascertained. To determine the range of contraction and expansion respectively, the limits selected are directed to be assumed in succession, that of contraction in the first instance, after which the tapes are accurately adjusted by touching the springs, and a record made of the result. The limit of thoracic expansion determined upon is next assumed, and by that movement the tapes are drawn out on each side to the extent exactly of the increase in girth acquired by the corresponding side. This process may be again and again repeated to insure accuracy of result, by simply touching the spring with the thumb or finger-nail, and thereby tightening the tapes preparatory to a fresh movement of expansion. The figures now registered for each side represent the range of expansion within the limits selected, whilst a comparison of these figures for the two sides will exhibit their relative expansibility. By inverting the order observed in the last-mentioned experiment the absolute and relative contractibility of the two sides of the chest may be similarly ascertained.

I think all the advantages of stethometry, for the purpose of diagnosis, may be attained by horizontal measurement of the chest at different levels. Vertical and oblique measurement can add very little to the

information thereby acquired, except in the case of incurvation of the spine; and as it must protract the exposure of the patient's chest, and increase the fatigue of examination, I would, save in exceptional cases, eschew it altogether.

I do not think, with a distinctly graduated tape-measure, dials insure greater accuracy, or that they are at all necessary; they are, moreover, objectionable, owing to the liability of the appended apparatus to get out of order.

Dr. HEAD.—It would be a pity to allow so simple and beautiful an application to pass unnoticed. I am sure the Society ought to feel obliged to Dr. Hayden for it. It is no doubt very simple, but its great beauty is its simplicity. We are all in the habit of measuring chests by a single line; but that cannot compare in accuracy with this method of measuring by a double ribbon. The single line can only measure the chest in one condition of respiration. This method applies to every condition of respiration; and it is certainly a very great addition to our means of forming an accurate diagnosis in some forms of disease of the chest. I think, therefore, that the Society ought to feel very much obliged to Dr. Hayden.

Dr. BURKE read the following paper on the present epidemic of small-pox:—

At the last meeting of this Association we had the privilege of hearing a most important and instructive paper read by Dr. Stokes on the Treatment of Small-pox. On that occasion, at the request of Sir Dominic Corrigan, I consented to prepare a paper on the Prevention of Small-pox.

On reflection I found that this task, which had been hastily undertaken, was surrounded with difficulties, the subject being one requiring to be dealt with by some person possessing an amount of knowledge and experience such as I cannot lay claim to.

Your secretary, Dr. Eames, has informed me that he could not find me a substitute.

I now, with much diffidence, propose making a few observations on the present epidemic; not that I have anything new or original to communicate, but in the hope of eliciting such information from our most distinguished and experienced confrères as may prove at this crisis instructive to the profession, and, therefore, of incalculable benefit to the public at large.

It is not long since that the cattle plague which had prevailed for some time in various parts of the Continent broke out in England. We all remember how great was the consternation produced thereby in this country. Public meetings were held in various localities; even Boards of Guardians bestirred themselves—deputation after deputation waited upon

the then Lord Lieutenant (the Earl of Kimberley), urging upon him the danger the cattle would incur should a strict quarantine not be enforced. In consequence of these pressing appeals His Excellency took instant action, and with the happiest results.

A new Government department was added to the Privy Council Office, Dublin Castle, and at the head of this department was placed our most eminent veterinary surgeon, armed with great powers, and supported as necessity arose by a staff of inspectors.

The importation of cattle into Ireland was utterly prohibited—any animal which showed the slightest symptom of disease was immediately slaughtered and buried—the owner being allowed a certain compensation by the Government.

In consequence of the admirable sanitary arrangements as regards the bovine race, Ireland was spared from the cattle plague and an enormous commercial calamity.

For more than a year a loathsome disease has been epidemic in many parts of England and Scotland—thousands and thousands have fallen victims to this pest. Yet though the disease is well known to be about the most contagious, and though our intercourse with the sister countries is almost hourly, there have been no public meetings held with a view of protecting Ireland from this scourge. Have the landed proprietors, the farmers, the Boards of Guardians sent their deputations to the Lord Lieutenant to point out the imminent, and now present, outbreak of small-pox? No! Not till within, I may say, a few days has any action been taken. The plague in cattle excited intense alarm in the public mind—the plague in man has been looked upon with indifference.

All must admit that the administration of quarantine laws is surrounded with difficulties, especially in countries where commercial interests are most powerful. Yet all must equally admit that something should have been done. For example, the prevention of persons barely convalescent, or actually labouring under small-pox, from invading our shores—such should have been isolated. Again, the prevention of the importation of the clothes of those who died from small-pox. And here I may state that several reports have been received by the Registrar-General in which the outbreak of small-pox has been clearly traced to the transmission of infected clothes from England. As may be seen from those important lectures of Dr. Guy on "Public Health," at page 53, the "preventive measures adopted by Viscount John, the successor of Barnabo, in the year 1399, when the plague broke out in Italy for the 16th time," were far superior to any means adopted in Dublin in the year 1871!

For some years past Ireland has been blessed with a singular immunity from small-pox, as may be seen from the Registrar-General's Returns. The Compulsory Vaccination Act came into operation on the 1st January, 1864; in that year small-pox was epidemic in many parts of Ireland, and

the number of deaths registered therefrom amounted to 854; in 1865 the number fell to 461; in 1866 to 194; in 1867 to 20; in 1868, the number was 21; in 1869, 20; and in 1870, 32. The numbers for 1871 cannot for some time be known, but they must show a fearful increase.

In Dublin, in recent years, cases of small-pox have been so few (the deaths were as follows:—in 1864, 42; 1865, 71; 1866, 25; 1867, 2; 1868, 1; 1869, 1; 1870, 0) that some students have had no opportunities of studying the disease. As regards the hospital (Steevens') to which I have the honour of being attached, no case of small-pox had been admitted from the end of 1864 till the year just passed, 1871. The disease is now amongst us, the epidemic is a formidable one, and the members of our profession must endeavour to combat with it as best they may.

The first death from small-pox, registered in Dublin during the past year, occurred in Cork-street hospital on the 4th of March—it was that of a woman, aged 27 years, who had not been vaccinated. On the 11th of March a girl, aged 16 years, died in the same hospital; in this case there was no vaccination mark. *The first of these persons brought the disease from London.* The next deaths from small-pox occurred on the 15th and 17th of April; no reference to vaccination. On the 9th and 15th of May 2 deaths occurred—one case, a person stated to have been vaccinated, and one in which there was no reference to the subject. Seven deaths occurred in the month of June; 2 were of persons who had not been vaccinated, and in the remaining 5 cases there was no reference to vaccination. Four deaths occurred in July—1 not vaccinated, 2 instances not stated, 1 doubtful. Six deaths took place in August—1 not vaccinated, 4 no reference, and 1 doubtful. In September 14—1 not vaccinated, and 13 not stated. Forty-one occurred in October, of which 7 were of persons stated to have been vaccinated, 13 unvaccinated, and in 21 instances there was no reference to vaccination. Sixty-two persons died in November, of whom 13 were stated to have been vaccinated (one of them being a child who was vaccinated a day or two before the disease appeared), 19 were unvaccinated, 1 “indistinct,” and in 29 instances there was no reference to vaccination (of these 13 deaths occurred in the North Dublin Union Small-pox Hospital). One hundred and fifty deaths took place in December, of these 19 were of persons stated to have been vaccinated, 45 unvaccinated, 1 case was returned as “indistinct,” and in 85 instances (including 21 city patients who died in the North Union Small-pox Hospital) there was no reference to vaccination.

Up to the 13th inst., 75 deaths that occurred in January have been registered. Of these, 9 were of persons stated to have been vaccinated, 10 not vaccinated, and in 56 instances (including 25 which took place in the North Union Small-pox Hospital) there was no reference to vaccination.

Three hundred and sixty-five deaths have been registered since the 4th of March (including the city patients (59) who died in the North Union Small-pox Hospital). Forty-nine of the total deaths were of persons stated to have been vaccinated, 94 of persons not vaccinated; 4 cases were returned as doubtful or indistinct, and in 218 instances (including the 59 deaths in the North Union Small-pox Hospital) there was no reference to vaccination.

249 of the 365 deaths occurred in hospitals, and 116 at the patients' residences. The greatest number of deaths in one day occurred on the 4th of January, viz., 14. Of the 365 persons, 22 were under 6 months old; 3 from 6 to 12 months; 7, 1 year; 10, 2 years; 6, 3 years; 4, 4 years; 14, 5 years; 10, 6 years; 11, 7 years; 6, 8 years; 5, 9 years; 49, 10 to 14 years inclusive; 48, 15 and under 20 years; 63, 20 and under 25 years; 77, 25 and under 35; 24, 35 and under 45; 2, 45 and under 55; 3, 55 and under 65; and 1 was a man aged 70 years, who was unvaccinated; he died, as the report states, from "variola maligna" of two day's duration in Cork-street hospital, on the 20th of December; he was brought from 1, Bonny's-lane. One hundred and eighty-eight of the deaths, or 51.5 per cent. of the total were of persons between the ages of 15 and 35.

As to the prevention of small-pox, the sanitary rules applicable to all diseases apply to this. In this city there has been such an utter neglect of sanitary provisions that it is only surprising that the present epidemic of small-pox, spread as it has been in all directions, has not produced more fatal results.

I have seen a great deal of the poor in this city, and do so occasionally now; and none but the clergy and dispensary medical officers can give you any idea of their position. A filthy room, crowded with occupants, with a case of small-pox in their midst, kept there from the horror of the people at the idea of the "sheds." Now the sheds, as you all know, mean the hospitals for small-pox connected with the North and South Dublin Union Workhouses; these sheds had been used on former occasions for fever and cholera, and there are few of the poorer classes who do not recollect having lost relatives therein.

Well, to escape going into the sheds, as I have been informed by Roman Catholic clergymen, the people will by every possible means conceal the nature of the disease under which they suffer. Now I would suggest that if no other places can be found for the accommodation of those labouring under small-pox, some other name should be adopted; in this, I think, our dispensary physicians and surgeons will agree with me.

We now come to a very important question, and one which has been frequently discussed and referred to in the press, I mean the conveyance of patients to hospital. At the present moment, I venture to assert, there is not sufficient accommodation afforded, and the difficulty

of procuring small-pox vehicles is great. Then again, when you get your patient into hospital, if he dies he is, perhaps, as a corpse, taken away to Mecklenburgh-street, or some other street, and "waked." As regards the special case of the wake in Mecklenburgh-street, the Rev. Mr. O'Neill informed me of it; and since the period of the wake referred to by this clergyman, every week deaths from small-pox have occurred in this street, and in the streets immediately adjoining. There are no legislative enactments to prevent the holding of wakes, but I am happy to say that His Eminence Cardinal Cullen has pronounced strongly against them, and I trust he will, by this means, have aided materially in preventing the spread of small-pox.

The next question suggested is what are you to do with the convalescents; they cannot be kept in hospital, for they must of necessity give place to acute cases, and even were there accommodation for them, their convalescence would be retarded by a prolonged residence therein. This matter has been taken up by the authorities, but the proposed site for a convalescent hospital is one that could not for obvious reasons be entertained. There should be (I am taking a great liberty in suggesting it) several moderate-sized houses taken for the purpose, and such a plan would be far better than aggregating a large number of small-pox convalescents in one building.

I now come to *the* great preventive of small-pox, a preventive that has not diminished in any way since its first introduction.

An idea prevails to some extent that the protective powers of vaccination against small-pox have failed. The absolute protection from small-pox by vaccination cannot for a moment be maintained—nor can the absolute protection of one attack of small-pox be maintained, since we have many cases on record of persons who have succumbed to a second attack. This is however beyond all question true, viz., that in the great majority of those who had been properly vaccinated, and in whom the true Jennerian vesicle had been produced, followed by the characteristic and unmistakable cicatrix, when they suffered from small-pox the disease was modified. The reports received from medical men in all parts of Ireland are in accordance with the foregoing.

There has been an absurd outcry raised against vaccination, by some who state that such a practice is "flying against the providence of God," and others object to the operation because they allege that other diseases may be introduced with the vaccine virus.

As to the first class of objectors to vaccination it is needless to deal with them. With regard to the introduction of diseases by vaccination *properly* performed, I have only to refer to the information obtained and carefully analysed by the distinguished medical officer of the Privy Council, Mr. Simon.

Mr. Simon, in the year 1857, put the following query to 542 members

of the profession in England, Ireland, and Scotland—"Have you any reason to believe that lymph, from a true Jennerian vesicle, has ever been a vehicle of syphilitic, scrofulous, or other constitutional infection to the vaccinated; or that unintentional inoculation with some other disease, instead of the proposed vaccination, has occurred in the hands of a duly educated practitioner?" The answer was almost unanimously "No!" from the 542 medical men, including the most eminent in the three kingdoms. Similar testimony was given by the most illustrious physicians and surgeons of Paris and Vienna. Again, of the 151,316 (adult) vaccinations in the British Army, no case of a soldier having been syphilized has come to the knowledge of the eminent head of the Statistical Department of the army, Dr. Balfour. Does the lymph lose its power by passing through a great number of individuals? No, for young cows can be vaccinated from human beings—the regular pock forming, and the lymph unchanged by passing through the cow will produce, by what is termed retro-vaccination, the true Jennerian vesicle. Does the protective power of vaccination wear out with years? I think not. A frequent cause of the non-protective powers of vaccination is the carelessness with which the operation has been performed, and another, the unskilfulness of many who have embarked in practice perhaps utterly ignorant of the appearance of a true Jennerian vesicle. I speak now more of the past than the present. Now instruction is given to vaccinators—formerly it was not so; and I will venture to state that at the period (a long while ago) when I had completed my curriculum, a very large per-centage of the newly-passed surgeons (I include myself) knew very little about vaccination.

The principal subjects to be discussed by the members of this association, in relation to the present epidemic, are, first, what are the best means to be had recourse to for the purpose of checking the spread of the disease? and this embraces, the conveyance of small-pox patients to hospital, how to deal with the convalescents in case of recovery, and in fatal cases as to the burial of the dead. Again, at what period after the cessation of the disease is the individual free from infection? a question very frequently put to medical men. And lastly, the very important question of re-vaccination.

As I have an idea (Dr. Burke added) that the protective power of vaccination remains, it behoves me to ask for information from those who know most about re-vaccination and its necessity. I have frequently been consulted by friends and others as to whether they should be re-vaccinated or not. I have asked to see the arm of the person who consulted me, and if I found on it a good mark—a white cicatrix, with a centre like a thimble—I have declared myself to be not for re-vaccination. But I have at the same time said—"If re-vaccination will give you confidence, which is a very important thing in meeting with any disease, by

all means do so." Sir, I thank you and the rest of the audience for your kind indulgence in listening to what you all know as well as myself.

Dr. DARBY said, I have had a great deal of experience all through my medical life of small-pox epidemics, being in charge of a public institution; and as far as my experience goes I am very much disposed to coincide with everything that Dr. Burke has stated. I will begin with the last observation that he made. I think it has been rather a reproach of the Dublin School that they have not insisted on every man entering the profession taking a certain short course of study or instruction in connexion with a cow-pock institution. I may say for myself that I learned vaccination because I happened to have a friend at a cow-pock institution. If I had not had that friend I never would have known a single thing about it until I began to practice, except what is given at the lectures; and therefore I think it would be a most desirable thing if every student were to take some instruction at a cow-pock institution. With regard to the question of the importation of the disease, I think it a most difficult question, in fact so difficult that I never could form an opinion myself on it. I open my eyes and cannot avoid seeing what comes before them in my own hospital. There is a patient in my hospital suffering from small-pox, and I will give you the history of her case: She came from a neighbourhood where there is no small-pox. She is a married woman, the mother of two or three children, and was in a bad state of health prior to her confinement. She was pregnant and delicate, and did not go out. Her cottage is in a very healthy part of the country, and is isolated. She was confined to her room, and remained delicate after her confinement; did not go out, and had very little intercourse with her neighbours. Two months after her confinement she came to my hospital in small-pox, with an unvaccinated infant in her arms. After she was admitted I vaccinated the baby; and on that subject I will just mention incidentally that four days after the child was vaccinated it showed pustules on various parts of its body, and ultimately had a large crop of ill-formed mild pustules over its whole body from head to foot. The mother had positively never been vaccinated; she had confluent small-pox in her face, and confluent small-pox on her arms, and also pretty generally over her legs and trunk; and she eventually recovered from what I would call a moderately severe attack. The baby has run through that peculiarly modified form of disease. Let me mention another case:—In the year 1848, or in 1846—I don't exactly remember which—we had an epidemic of small-pox. Two or three years before that a boy who had been blinded by natural small-pox, and who was very much disfigured and maimed by the original attack, came into the institution with which I am connected. He was so very bad, that though he was

only seventeen years of age, I had him placed in the infirm ward amongst the old men. He was there for at least two years without having ever gone out. That boy, as far as my information and knowledge goes, and I took pains to ascertain the fact, was the first person in the Rathdown Union who took the small-pox. He took it in the infirm ward of the work-house; and the attack was a most virulent one, of which he died. It was secondary natural small-pox; and he had had no intercourse with anyone that could by any possibility have conveyed the infection to him. He had been for the two preceding years imprisoned within the walls of the ward, and was scarcely ever out of his bed. I have met other cases somewhat similar; and my experience is, that whatever importance you may attach to contagion, or to the disease being imported from one place to another, there is another influence also at work. I know nothing about contagion; I won't deny it. I have seen families sometimes taking the disease and sometimes not. I am at the present moment attending a child ill of small-pox in one of the dirtiest houses perhaps to be found in Ireland, belonging to a comfortable person. It is a small cabin, the floor of which is lower than the dung-pit attached to it; and there is water and ooze coming through the floor. The man has seven or eight children. One of them is going through small-pox; and up to the present it has not spread to the others. He will not send them out of his house, and he has no means of separating them. These cases are to be taken for as much as they are worth; but from the circumstances I mention to you, I do not attach so much importance to contagion as a means of importing the disease as others do; and I am quite as certain as I can be of anything of the kind, that whether there be contagion or not, there is another influence superadded over and above contagion; that all diseases may arise spontaneously, and that you cannot foresee, in any intelligible or satisfactory way, the cause of its coming to certain places.

Dr. ROBERT M'DONNELL.—I am anxious to say a few words, because this subject is one in which I take a good deal of interest. Last year, when I saw this epidemic approaching, I ventured to make some suggestions, which, I have now very great reason to regret, were not adopted. The reasons which led me to make these suggestions were these. As Medical Superintendent of Mountjoy Prison, I had an opportunity of ascertaining, with some precision, what number of persons in this country are vaccinated, and what number are not. This is a very important fact for us to get hold of in the first instance with regard to any country. We have not very long had a registration in this country; and therefore we have not the means of positively ascertaining, with anything like scientific precision, how large a number of people in this country are vaccinated, and how large a number are not. For some considerable time I inspected all the prisoners admitted into Mountjoy, as was my duty

to do. They are stripped naked for the purposes of that examination, and therefore I had an opportunity of seeing whether they bore evidence of having been vaccinated, or had had the small-pox. I took the trouble to-day of looking over the returns with Dr. Young; and I find that I have notes made in this way of between 900 and 1,000 persons taken, rough and smooth, all over Ireland, and taken, of course, from the humbler ranks of life. We may say, therefore, that these 950 persons give, in rough figures, the condition of the country as regards vaccination. I found that, in the rough, 75 per cent. of these persons were vaccinated; and that of those who had not been vaccinated a very large proportion indeed were marked as having had the small-pox. The exact figures I could easily make out, but I may say that from 19 to 20 per cent. of those who had not been vaccinated had had natural small-pox in some way or other. Of course I am not able to say how far they got it from inoculation, or how far from the ordinary modes of infection. I can only say that 20 per cent. of the persons who passed through my hands in Mountjoy Prison were marked with small-pox. Assuming that to be the state of facts, I knew very well that, though a great lull had come on small-pox, and though the authorities were very well pleased at the perfect success with which small-pox was said to have been stamped out in Ireland, there must have been a very considerable proportion of adults in this country who, when the epidemic came to our shores, were only too ready to receive it. Feeling that, I suggested, in my letter to Sir Dominic Corrigan and by word of mouth, that active measures should be taken before the epidemic came. The suggestions I made were partly these:—First of all I proposed that the Guardians should be communicated with throughout Ireland, and that the clergy of all denominations should be communicated with, believing that they would lend a hand in the matter, with a view to pressing on the public the necessity of vaccinating the younger adults and children; and I urged that no children should be permitted to go to any schools without having been vaccinated. And further, as Dr. Burke has said, that very vigorous action was taken with regard to the cattle plague, so I suggested that although human beings could not be killed, yet we could kill the clothing of those who had had small-pox; and that the clothing of the poor which should have to be destroyed under those circumstances in any localities in which the small-pox appeared could be paid for at a very moderate expense indeed, much beneath the expense which people readily went to in order to repay the persons whose cattle were killed; and that that would be a very great inducement to the people to go to hospital. Lastly, and most of all, I proposed that the vaccination fees of the medical officers throughout Ireland should, for some time at least, be increased. The vaccination fee is fixed at present, by Act of Parliament, at a shilling. In the poorer districts of Ireland, where the shilling is an important

addition to the medical officer's salary, he works hard and makes some money by it, and his district is thoroughly well vaccinated; in fact it pays him to do it, and he does it well. In other districts the shilling is too little. It is not enough to induce the medical officer to take the trouble of vaccinating the people; in many places it hardly pays his expense of driving about his district. I proposed, therefore, that for a time it should be increased to—say half-a-crown; or, if necessary, to five shillings. That would have made it worth the while of the dispensary doctors to look up all the cases of adults, as well as children, in their districts. If he had had the co-operation of the parish clergyman and priest, he would have been able to get a great number of persons who had never been vaccinated to submit to that operation; and thus he would have had a sufficient interest in it to have worked with extreme care and vigour. The answer which Sir Dominic Corrigan gave to me when I wrote to him on the subject was that the vaccination fee was fixed by Act of Parliament. I replied that there was a panic in the land at the time, and that if an application were made to the Chief Secretary about the fee, or a movement made about it, it would be very easy to get the fee increased, temporarily at least; and that if during a period of three or six months it were increased to half-a-crown, there would not be a better vaccinated country than Ireland. We then would have been prepared with our armour for the attack; and when we would not spare an expenditure of millions to resist a foreign foe, it would not have been throwing away money to have spent a few thousands to resist an epidemic such as we have raging amongst us now! The calculation I made was that £20,000, expended in half-crown fees, would have rendered Ireland thoroughly well prepared; in fact something about the amount of the salary of the Lord Lieutenant would have put us into very good order, and would have saved many and many a life. I cannot help regretting extremely that these suggestions were not acted on at the time. I now approach the most important of all the subjects which Dr. Burke has touched on in his paper—namely, the prevention of the spread of contagion through the clothing. Vaccination is now unquestionably very actively carried out. The public are now frightened, and vaccination and re-vaccination are being carried out with extreme vigour. But I do think that there is still great room, very great room, for efforts being made by urging the people to go to hospital, and then destroying their clothing and paying for it, to prevent the contagion from spreading; and I cannot help thinking that it is still our bounden duty to urge this very strongly indeed upon the authorities.

Professor MOORE.—Last Spring, having been put on the small-pox committee of the College of Physicians, I wrote to the principal medical officers of every important town in Ireland, and received answers from

every one of them, with perhaps one or two exceptions. I speak *apropos* of what is called contagion, or the carrying of small-pox from one place to another. In March last I received a most interesting letter from Dr. Boxwell, of Wexford, in which he stated that they had then some 30 cases of small-pox in that town, and that he could distinctly trace the disease to the arrival there of a navvy from England. The man came there about January, and afterwards they had, he said, first 12 or 14 cases in one part of the town, and then 12 or 14 more in another; and that the disease was then flourishing in the town, and there had been a very considerable number of deaths. About the same time I was asked by Dr. Hughes to see a case, at the Mater Misericordiæ Hospital, of a woman, which was also a case of direct importation of the disease. She had come from Liverpool, where she had been washing out the apartments of soldiers. We meant to have kept on the action of that committee; but some very wise gentlemen pooh-poohed the whole thing, saying that we would not have any small-pox here. Eventually the labours of that committee came to grief. Then small-pox began to be rife here. Afterwards I had a very interesting communication with Dr. Gairdner, Sanitary Officer of Glasgow. He referred me to Dr. Dunlop, Inspector of Vaccination for the Privy Council; and I never corresponded with any gentleman whom I found more ready than he was to give all the information in his power. He mentioned that there had been 600 or 700 cases of small-pox in Glasgow; and that on a particular day they had a certain number of deaths. I don't remember the figure, but it was something very large indeed. He also mentioned Dr. Russell; we all know who Dr. Russell is—a man who, I may safely say, has seen more fevers than any other medical man in the United Kingdom; and stated in his letter that Dr. Russell told him that whenever he had reason to expect an invasion of small-pox, he, as soon as possible, vaccinated every one he could, and that *took off the edge* of the disease. That struck me as a very interesting expression. In relation to re-vaccination, Dr. Russell stated that he had been taken about a fortnight previously to a convent in Glasgow, containing 150 inmates, of whom 14 had small-pox. He removed the 14 patients, and re-vaccinated the balance of the inmates; and at the end of three weeks no additional case had occurred there. I had the impertinence to read a letter on the subject of vaccination before the British Association. At that time we had no Registration Act for Ireland, and did not not know how many or how few of the people were vaccinated. We had dispensary returns which showed that there were a great many persons unprotected. I recommended in my paper that parents should not let their boys go to school unless they had been first vaccinated. When our Registration Act came into force I thought that all that would have become unnecessary. Let us come to another phase of the subject.

I have not seen more than 50 cases. Of these I have notes of 37; and in some of those 37 cases the phase of the disease was very severe. I saw a case some few weeks ago, which was also seen by Dr. Stokes, and he said that he had seen nothing so severe in his experience.

I think the premonitory symptoms are to a certain extent indicative of what the disease will be. Of course so long as patients were delirious you could find out nothing about them; but when they became conscious I inquired when the eruption was first noticed; and in all the most severe cases I found that it occurred from 24 to 36 hours after the premonitory fever. In some of the cases it followed immediately after the fever. In some cases, also, the symptoms of severe pain in the back and vomiting were even preceded by delirium; and I found that delirium in the beginning always meant a very severe type of the disease. The delirium in some of the cases was exceedingly severe—it was *delirium ferox*. The character of the eruption kept pace with the premonitory symptoms, and the latter, in some of the cases, occurred a little out of order. For instance, in one case that I admitted to Sir Patrick Dun's Hospital from Summer-hill, the entire body was covered with blotches, while there was perfect consciousness and little apparent fever: and in that case there was considerable bleeding from the gums, nose, and eyes—in fact everywhere. In 36 hours that case proved fatal. I cite it as an example of one of the severest hæmorrhagic types. The eruption in that case was not at all remarkable. I have only a word to say as to the spread of the disease. In Sir Patrick Dun's Hospital we could not accommodate more than 22 patients. After about 10 of these got convalescent and could eat and drink, they became very restless. I saw them turned out with their clothes, and of course they carried their clothes with them everywhere. That shows the great importance of establishing a convalescent hospital. If those people had been afterwards located in different places during the period of their convalescence, it would have tended immensely to check the spread of the disease, because it came under my observation that in two cases where members of families went home from the hospital the disease extended over the families.

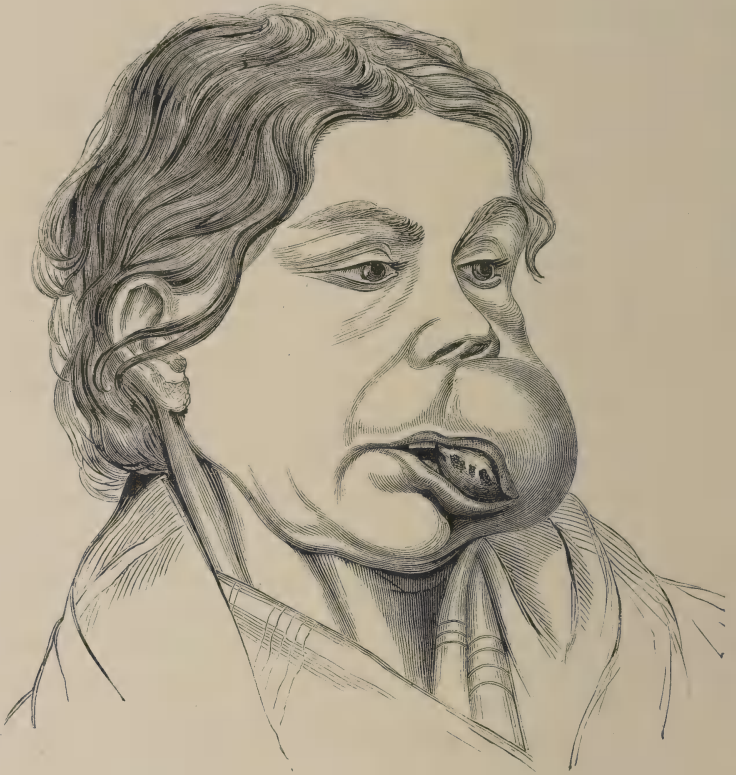
Dr. LALOR.—Allow me to make a remark. I had a conversation today with a medical brother whom we all know, in which he mentioned that he had vaccinated and re-vaccinated 2,000 cases; that he had found that in houses where there was small-pox those who had either been vaccinated or re-vaccinated invariably were not attacked, and that those who were unwilling to undergo the operation of re-vaccination, or had not been vaccinated originally, invariably were attacked with small-pox. In reference to the dangers that some apprehend from re-vaccination, his experience was that where proper precautions were taken he never

saw a single instance of any dangerous or even troublesome symptoms. The precautions he considered to be necessary were, that you should not vaccinate persons who from the state of their constitution or the state of their health are unfit for vaccination, and in the second place, that you should be most particular about the matter with which you vaccinate, particularly with regard to the period at which you take the matter from the vaccine vesicle. The eighth day he stated to be the day on which, other circumstances being equal, you ought to take it. One great thing, he said, that ought to be kept in mind was the appearance or non-appearance of the areola. The areola sometimes appears before the eighth day; and he stated that whenever that occurred you ought not to take lymph from the vesicle. His opinion was that the areola indicated a sort of suppuration, and that once the lymph had passed into that state it was dangerous to attempt to vaccinate with it. I make these observations, because I may not be able to attend on the next evening; and when I mention the name of Dr. Osbrey as the gentleman from whom they come, the meeting will recognize in him one of those modest and retiring, but thoughtful, men, whose observations, when they come before the profession—and they come but too seldom—are entitled to more than usual weight.

CHAIRMAN.—It is proposed to adjourn this debate until next Wednesday. Would it not be well to include the question of treatment at the next meeting? It is not the least important part of the subject.

Dr. HEAD.—If you include treatment you will have to adjourn the question again. We are still asked questions so often about the prevention of the disease that I think it would be very desirable to concentrate the knowledge that we possess on that point, so as to give to each individual the benefit of the experience of others. It is very hard to decide upon one's own experience alone; and I think it would be very important if we could ascertain from the experience of those gentlemen who have been engaged in vaccination the number of well vaccinated cases, and apparently with a good mark, that have taken vaccination a second time. If those gentlemen who have the care of large small-pox hospitals will give us their experience as to the cases they have seen, and the character of the disease that followed undoubted vaccination and re-vaccination, that will occupy a great deal of time, and probably the attention of more than one meeting.

Dr. BEATTY.—As I may not be here when the Society meets again, I wish not to leave the room without stating that I have been very extensively re-vaccinating for the last couple of months. Many of these were persons whom I had vaccinated in their youth, for I have lived



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long enough to see another generation spring up under my care. And I have been surprised at the way in which persons—generally boys and men whom I had vaccinated, and who bore the most splendid marks, specimens that they would take to their graves of successful vaccination—have taken re-vaccination, and at the vesicles, perfect as those you would see on an infant, that have formed on their arms. In many cases of adults, aged from twenty to thirty, I have seen the most perfect vesicles formed; and in the cases of younger persons the vesicles have been so beautiful as to deserve to be drawn as specimens of the disease. I have re-vaccinated freely whenever I have been asked to do so, and I never saw a single instance of a bad result from re-vaccination. I have seen what are called sore arms, particularly on persons aged from twenty-five to thirty and thirty-five, where the vesicles did not form as well as in other cases; and a sort of spurious inflammation, or extension of inflammation round the wound, but nothing of the slightest importance, or that a wet rag did not put an end to in a couple of days. But I never saw a single bad result arising from re-vaccination. And the grand point is that of giving confidence to the individual, when you can at so little expense satisfy his mind and moreover satisfy your own mind; because if you refuse to vaccinate a person who asks you to do it, and that person subsequently gets small-pox, I need not tell you what you will get.

The meeting was then adjourned.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

Tumour of the Superior Maxilla.—MR. EDWARD HAMILTON exhibited a tumour of the upper jaw, which had been the subject of operation that morning in Steevens' Hospital. The patient was a woman sixty years of age. The tumour projected very largely in front, bulging the cheek forward. It also projected into the mouth, extending a considerable distance below the margin of the gum; an outgrowth of the tumour protruded into the nostril, and could be detected by sight as well as by touch. It was also found that it passed across the mesial line of the roof of the mouth, and extended a considerable distance outwards over the malar bone. The history of the case was as follows:—She always enjoyed good health; but about six years ago suffered from a small tumour on the gum of the upper jaw. This gave her some pain, though not very much. It continued to grow for nearly a year, after which time she was admitted to the Meath Hospital, under Mr. Porter, who, at that time, removed a considerable mass from the upper jaw.

She remained free from any signs of disease for about two years, and

then began again to complain of some uneasiness in the upper jaw, and a feeling as if the disease were about to return. A very careful examination was made of the case, and he and his colleagues came, for the following reasons, to the conclusion that the disease was probably not malignant—the slowness of its growth, the absence of glandular contamination, and the fact that she showed no trace of constitutional cachexia. They, therefore, considered the case a favourable one for operation; and having regard to the fact that the disease had recurred, it was determined to give the woman a better chance of complete recovery, by removing the whole superior maxillary bone. Accordingly, the operation was performed in the usual way—the ordinary incision and section of the three main bony points were made, and on pressing the tumour down, to dislodge it through the mouth, the entire mass was removed with the greatest possible facility. It almost dropped from its place as if enucleated.

The growth had a soft elastic feel, conveying a deceptive sensation of fluidity. It appeared to have had its origin from the alveolar arch, the last molar tooth being imbedded in it; from thence it passed back into the antrum, which it completely filled, and projected through its openings; on section it was smooth, uniform, and glossy, having the appearance of a fibro-plastic growth. Under microscopic examination it exhibited abundance of fibres, with a few cells scattered through them.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

A meeting of this Society was held in the College of Physicians, Kildare-street, on the 20th of January, 1872.

Dr. SIPTHORPE in the Chair.

The following paper was read by DR. JOHNSTON, Master of the Rotundo Lying-in Hospital:—

MR. PRESIDENT AND GENTLEMEN,—In presenting to you these reports of the great institution of which I have the privilege and honour of being Master, I would wish to impress upon your minds that my great and primary object in doing so is, and has always been, not to advocate or put forward any particular theory or principle of my own, but by simply and unprejudicially laying before you a plain and correct statement of facts as they occurred, fairly to test the question, whether large internal maternities are, or can be made, safe asylums for lying-in women.

In the first place, allow me to say that, during the past twelve months, the healthfulness of the hospital has been severely tested, inasmuch as—



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and, of course, you must be all well aware—epidemics of various kinds, and of a serious nature, such as typhus and typhoid fever, erysipelas, scarlatina, and latterly small-pox, have from time to time prevailed, to a considerable extent, in various parts of the city and suburbs, while the hospital—although admitting patients, not only from localities where the diseases existed, but absolutely from among families, many of whom had died, or were suffering from the epidemic,^a and some even had the disease actually upon them when admitted—I am happy to say continued healthy, as in no one instance did the malady extend beyond the individual so affected; thus evidencing a strong proof that a large maternity can be fully as safe, if not safer, than allowing patients to be confined in their own homes; that it does not favour the engendering of disease or the spread of contagion (as is the idea of some); and corroborates the opinion I had at first formed, which was, and is still my firm conviction, that “where perfect purity of atmosphere is preserved, by not over-crowding patients together, allowing ample cubic space for each individual—where thorough ventilation is established, and strict cleanliness is in every particular maintained—when labour is not allowed to run too long, *i.e.*, to the verge of inflammation, and a generous diet is given in a judicious manner from the commencement according to the circumstances of the case—where all these particulars are carried out in rigid detail, it is quite possible to keep patients in a healthy state, and that it is not likely that anything of an endemic nature will pervade the wards even of a large maternity.”

In thus speaking, be it remembered, I do not for a moment presume to maintain that epidemics can be prevented from entering an hospital, no more than the mansions of the rich or the hovels of the poor; but what I contend for is that it is perfectly within the bounds of possibility that we can render hospitals, and even large ones, as safe as private dwellings, and certainly more so than the wretched filthy rooms from whence come the majority of those seeking the advantages and benefits that such institutions as the Rotundo Lying-in Hospital affords.

During the year terminating the 5th November, 1871,

There were 1,161 intern labour cases, beside which

181 cases were delivered at their own homes,

4,028 attendances at the dispensary, and

203 were under treatment in the chronic ward.

Making in all 5,573 cases relieved in the year.

Of the 1,161 intern labour cases (403 were primiparæ and 758 were pluriparæ) 963 were purely natural labour, *i.e.*, the head presented, and the labour terminated by the natural efforts within twenty-four hours;

^a In one instance four children died of scarlatina.

19 were tedious, *i.e.*, the labour lasted upwards of twenty-four hours, but did not require artificial aid, the delay in all being in the first stage; 24 were abortions, *i.e.*, the ovum was expelled at periods from the 1st to the 6th month; 56 were præternatural cases, 8 of which presented with the shoulder, elbow, or hand; 48 where the breech or lower extremity was the presenting part; 64 cases had to be delivered with the forceps; 6 instances where craniotomy had to be performed, and delivery effected in 2 with the crotchet, and in 4 with the cephalotribe. In 5 of these cases there was distinct evidence as to the death of the child before operation; in 1 it was doubtful.

In 39 cases the labour was complicated, viz., 16 being twins, 8 were cases of accidental hæmorrhage, 1 of placenta prævia, 5 post partum hæmorrhage, in 9 cases the placenta was retained by morbid adhesions; there were 8 cases of prolapse of funis, and 1 rupture of uterus, 2 convulsions, 1 epilepsy; mania occurred in 12 instances; in 55 chloroform was administered.

There were 25 cases of peritonitis, 1 of puerperal fever, 1 of metritis, 5 of phlebitis, 4 of pyæmia, 4 of sloughing of vagina and perinæum, 1 of gangrene of the uterus, 5 of scarlatina, 1 of variola, 3 of erysipelas, 2 of typhoid and 1 of typhus fever; 36 cases of bronchitis, and 1 of pneumonia.

Besides the foregoing list of casualties, and in order to demonstrate more clearly the full amount of accidents (if I may so call them) this institution is liable to—where all seeking admission, even the most abandoned, may freely enter without any previous examination, or inquiry into their circumstances, the only recommendation being that they stand in need of our assistance—I think in justice to the question (as to the safety or otherwise of large maternities) it is but right and fair to lay before you the following list, showing the *condition* in which the several patients presented themselves *on admission*; and, as I deem it unnecessary to detail the number of healthy individuals, I will merely enumerate those actually suffering under the various maladies, or who had been exposed to untoward circumstances previous to their coming under our charge.

There were 5 cases of what I may call malpraxis, viz.:—In two instances the forceps had been tried several times ineffectually before being brought to hospital; 2 were under the care of a woman for 24 and 33 hours respectively; the fifth we were not able to ascertain the length of time, but the arm was protruding through the vulva, and the waters had escaped eight hours previous. 7 cases where the child was born for some hours before admission, and therefore the mothers were exposed to all the dangers of hæmorrhage, inflammation, &c. 47 were in very delicate health, suffering from great debility, either from severe sickness of stomach during pregnancy,

recovering from fever, labouring under anasarca, extreme emaciation, fretting with great anxiety of mind from seduction, ill treatment, deserted by, or death of husband, &c.; 8 cases of exhaustion from accidental hæmorrhage, 1 from unavoidable hæmorrhage, 36 from bronchitis, 2 from pleuritis, 3 from phthisis, 1 pneumonia, 2 from laryngitis, 1 tonsillitis, admitted from another hospital; 3 inflamed varicose veins, 1 erysipelas of head, 3 injuries received by beating, 1 influenza, 1 hæmoptysis with peritonitis, 1 jaundice, 1 epilepsy, 3 condylomata, 1 secondary eruption, 3 purulent vaginitis, 1 variola, 1 hysteric convulsions, with hæmorrhage in the 3rd month, 1 typhus fever.

It will be observed that our mortality, amounting to 33, is considerably greater than either of the former years, still, on examining minutely into the circumstances of the deaths, it will be clearly seen that they could not in any way be attributed to the air or atmosphere of the hospital; that in fact these deaths would have taken place had they been confined in their own homes, or even in cottage hospitals or detached buildings, if such was the nature of the shelter they had sought.

And in order to show this, I beg leave to draw your attention to the following table, No. 1, giving a list of the deaths that occurred from all causes during the twelve months; they are registered chronologically, as in my former reports, with the wards in which they were confined and the beds to which each patient was conveyed after their labour terminated; so that it may be seen that the sickness of which they died, if of a zymotic nature, did not extend beyond the individual so seized.

The number of deaths, as I have before stated, amounted to 33—viz., 1 was brought in in a state of great exhaustion from placenta prævia, died in 8 hours; 2 in a feverish state, sloughing of vagina followed, one was under great distress of mind, and had fever a short time before; in the other the forceps had been several times attempted before admission; 2 cases of sloughing of vagina, peritonitis followed, both were innupta; 2 cases of sloughing of vagina with pyæmia, one was innupta, the other was in delicate health for three weeks previous; 8 cases of peritonitis, 4 of which were innupta, 3 were labouring under great distress of mind, and 1 had been in labour under care of a woman for two days before admission; 1 case of pyæmia, innupta, great distress of mind; 2 cases of phlebitis, one had inflamed varicose veins on admission, the other in very delicate health, attacked with mania on fourth day from bad news, when phlebitic inflammation followed; 1 case of typhoid fever; 1 of scarlatina, an innupta; 1 case of erysipelas, had been under care of a medical man for some time before admission, a delicate woman, got bad news on the ninth day, erysipelas over nates and hips soon developed itself; 4 cases of bronchitis, 1 of which had pneumonia, in 2 peritonitis followed, 2 of these were innupta; 1 of phthisis; 1 with hæmoptysis and peritonitis on admission; 1 of pneumonia.

TABLE NO. 1.—Deaths from all Causes, from Nov. 6, 1870, to Nov. 5, 1871.

No.	Date of Death	Ward	Bed	Age	No. of Pregnancy	Cause	Observations
1	Nov. 6	7	Back Ward	40	10	Complete placenta prævia	Admitted perfectly anæmic; version; draining continued; died in 8 hours.
2	" 7	5	44	30	1	Sloughing of vagina, hectic	Admitted very delicate, having had fever a short time before; fretting; "husband away."
3	" 12	7	60	30	1	Sloughing of vagina, hectic	Admitted very feverish; tongue dry; pulse 100; forceps had been applied four times before admission.
4	" 17	1	2	22	2	Typhoid fever, dysentery	Admitted with low typhoid fever; diarrhœa; enteritis.
5	" 17	2	17	22	1	Bronchitis, pneumonia, peritonitis	Admitted with acute bronchitis and pneumonia; great dyspnoea; peritonitis supervened.
6	" 20	6	70	20	1	Peritonitis	Mental distress, having separated from her husband.
7	" 26	12	113	33	1	Peritonitis	<i>Innupta</i> ; strangled her child evening of 3rd day; peritoneal symptoms morning after; died in 48 hours.
8	" 30	4	39	28	5	Peritonitis	Had been in labour under care of a midwife for 2 days before admission; had been in delicate health for some months previous.
9	Dec. 16	3	24	23	2	Phlebitis	Admitted in delicate health, with inflamed varicose veins; labour natural, 14 hours.
10	" 19	4	42	21	1	Peritonitis	<i>Innupta</i> ; fretting; very silent; symptoms set in 48 hours after delivery, which was natural, 5 hours duration.
11	" 26	5	44	26	1	Sloughing with peritonitis	<i>Innupta</i> ; sloughing of vagina, with peritonitis, appeared in 24 hours (labour tedious, 6½ hours in 2nd stage, by forceps).
12	Jan. 23	8	85	20	1	Sloughing with peritonitis	<i>Innupta</i> ; had been drinking from a pint to a quart of whiskey per diem for the last 7 months.
13	Feb. 1	6	70	30	1	Sudden	Died suddenly, 41 hours after delivery, by forceps; 6 hours in 2nd stage. P.M., clot found in left ventricle. Husband in America.
14	" 14	6	68	19	1	Bronchitis	Has been suffering for last 5 months; great dyspnoea; died in 59 hours after delivery.
15	" 14	12	115	22	1	Collapse	Rupture of a varicocele in left spermatic veins (labour 8 hours duration, 2½ in 2nd stage).
16	" 17	8	85	19	1	Bronchitis	<i>Innupta</i> ; 2 months ill; great dyspnoea.
17	" 24	4	39	23	1	Phthisis	Has been declining for last 3 months.
18	Mar. 6	2	16	27	2	Scarlatina	<i>Innupta</i> ; rash appeared on the 3rd day; died 48 hours after attack; labour lasted only 3 hours.
19	" 11	5	44	22	1	Peritonitis	<i>Innupta</i> ; fretting; mania, with peritoneal symptoms on 5th day; died in 48 hours.
20	" 25	3	22	28	4	Pyæmia	<i>Innupta</i> ; great distress of mind; bronchitis; pyæmic patch appeared on 5th day; died on 7th.
21	April 3	3	27	29	1	Bronchitis, embolism	Had bronchitis for 3 weeks; extremely wasted; great dyspnoea; died 39½ hours after delivery.
22	" 5	7	60	23	1	Peritonitis	Extreme depression of spirits from the commencement of pregnancy; dread of result.
23	May 14	1	2	23	1	Sloughing with pyæmia	Difficult labour from deformity; delivered with forceps; had been in delicate health for 3 weeks.
24	June 14	3	30	33	4	Phlebitis	Great delicacy of health; mania on 4th day from bad news; phlebitis followed; sank rapidly.
25	" 26	2	18	34	2	Bronchitis with peritonitis	<i>Innupta</i> ; bronchitis for some months; in very low spirits.
26	July 4	6	69	29	5	Peritonitis	8½ months, from over-exertion, fretting, and ill treatment by her husband, who had deserted her.
27	" 5	2	19	25	1	Peritonitis	<i>Innupta</i> ; extreme despondency.
28	" 12	4	42	25	2	Hæmoptysis, peritonitis	Great abdominal tenderness on admission; has had hæmoptysis, with cough, for some time.
29	" 12	5	49	18	1	Gangrene of Uterus	<i>Innupta</i> ; had fever a month since; in very delicate health; sank on 6th day.
30	" 22	7	57	19	1	Rupture Uterus	<i>Innupta</i> ; labour 60 hours; delivered with forceps; died on 2nd day; cervix uteri found very soft and pulpy.
31	Aug. 3	7	60	22	1	Sloughing with pyæmia	<i>Innupta</i> ; had been under medical treatment before admission.
32	Oct. 15	8	80	29	3	Erysipelas	Delicate health; admitted in 2nd stage; delivered with forceps; erysipelas on 9th day; fretting.
33	" 29	6	69	37	1	Pneumonia	Admitted suffering under bronchitis and pneumonia; breech presentation.

There were 3 died suddenly, 1 of clot in the left ventricle, 1 of rupture of a varicocele in left spermatic vein, 1 of embolism, 1 having had bronchitis for three weeks, 1 of gangrene of the uterus, an innupta, and 1 of rupture of uterus, also innupta, had been under treatment for "dropsy" for some time before admission.

It will also be found, and I wish to draw your attention to the fact, that of the 33 deaths, 13 were cases of *seduction*, in one of which scarlatina appeared on the 3rd day, 5 were suffering under great distress and anxiety of mind, 5 from extreme delicacy of health, 2 were admitted with all the symptoms of fever, 1 with phlebitis, 1 with phthisis, 2 with pneumonia, 1 with hæmoptysis and peritonitis, 1 had phlebitis on admission.

So that, taking these circumstances into consideration, we may safely say that in no one instance could the death, in any sense, be attributable to the patient having been confined in the institution, as the same might have occurred had their delivery taken place in their own homes (if they had such), or in the isolated wards of cottage hospitals.

You will also perceive that of these 33 cases of deaths from all causes, that 21 were of a zymotic type, viz. :—1 typhoid fever, 2 bronchitis, or pneumonia, with peritonitis, 1 hæmoptysis, with peritonitis, 2 sloughing of vagina, with peritonitis, 2 sloughing of vagina, followed by pyæmia, 8 peritonitis, 1 pyæmia, 2 phlebitis, 1 scarlatina, 1 erysipelas.

Again, in order to show in another point of view, and I think, more distinctly prove, that where deaths, particularly of a zymotic type, occurred, there was not contagion in any single instance. I beg leave to submit to your notice the following table, marked 2, of the number of patients delivered in each ward, with the number of deaths, as they occurred, in the several months during the year. You will perceive, on reading the table from left to right, it gives you the number delivered, with the number of deaths, in each ward during the month (deaths of a zymotic type are marked Z); by reading the table vertically, you will get the number delivered in each ward, with the number of deaths which took place during the twelve months.

Thus reading the table from left to right, you find that in the month of November, 1870, there were 9 deliveries and two deaths (zymotic), in No. 1 ward; 6 deliveries in No. 2 ward, and one death of a zymotic type; in No. 3 ward 9 deliveries and no death, and so on.

Reading the table vertically, you see that 122 deliveries took place in No. 1 ward in the year, out of which there were 2 deaths, the first of which was from a zymotic cause, admitted with low typhoid fever and diarrhœa, 17th November, 1870; the second, also zymotic sloughing with pyæmia occurred on the 14th of May, 1871, after an interval of nearly 6 months, during which 13 sets of patients were delivered; on 12 of which occasions the same bed was occupied.

TABLE No. 2.—No. of Patients delivered in each Ward, with the Deaths as they occurred in them, during the year ending 5th November, 1871.

		Ward No 1		Ward No 2		Ward No 3		Ward No 4		Ward No 5		Ward No 6		Ward No 7		Ward No 8		Ward No 12		Total		Deaths from zymotic diseases
		Delivers. Deaths.		Delivers. Deaths.		Delivers. Deaths.		Delivers. Deaths.		Delivers. Deaths.		Delivers. Deaths.		Delivers. Deaths.		Delivers. Deaths.		Delivers. Deaths.		Delivers. Deaths.		
1870																						
From																						
November 6th	- {	9	Z	6	Z	9	-	9	Z	9	1	9	Z	15	2	11	-	10	Z	87	8	5
December	- {	10	-	12	-	9	-	7	-	7	-	8	-	7	-	9	-	14	-	83	-	3
1871																						
January	- - {	8	-	7	-	11	-	13	-	11	-	10	-	9	-	10	Z	7	-	86	-	1
February	- - {	12	-	11	-	8	-	9	-	8	-	8	-	12	-	13	-	12	-	93	-	-
March	- - {	13	-	14	-	13	-	12	-	12	-	13	-	14	-	14	-	14	-	119	-	3
April	- - {	3	-	9	-	6	-	12	-	13	-	14	-	10	-	9	-	10	-	86	-	1
May	- - {	13	Z	13	-	8	-	13	-	13	-	9	-	11	-	15	-	11	-	106	-	1
June	- - {	8	-	9	-	9	-	8	-	8	-	12	-	13	-	12	-	13	-	92	-	2
July	- - {	10	-	12	-	12	-	12	-	13	-	9	-	8	-	8	-	9	-	93	-	3
August	- - {	11	-	12	-	9	-	9	-	7	-	9	-	12	-	14	-	14	-	97	-	1
September	- - {	7	-	8	-	13	-	11	-	9	-	12	-	14	-	11	-	8	-	93	-	-
October	- - {	13	-	10	-	14	-	14	-	12	-	9	-	10	-	11	-	13	-	106	-	1
To																						
November 5th, in- clusive -	- - {	5	-	1	-	-	-	-	-	-	-	2	-	4	-	3	-	5	-	20	-	-
Total Deliveries in each Ward		122	-	124	-	121	-	129	-	122	-	124	-	139	-	140	-	140	-	1161	-	-
Total No. of Deaths in each Ward		-	2	-	4	-	4	-	4	-	4	-	5	-	5	-	3	-	2	-	33	-
Total No. of Deaths from zymotic dis- eases		-	2	-	4	-	3	-	3	-	2	-	2	-	2	-	2	-	1	-	21	-

Deaths from all causes, 1 in 35½. Deaths from zymotic diseases, 21, or 1 in 55½.

In No. 2 ward, 124 deliveries took place in the year, and 4 deaths, all of zymotic character; the first, in November, admitted with acute bronchitis, pneumonia, peritonitis supervened; the next, a case of scarlatina, did not occur till the 6th of March, after an interval of nearly 4 months, during which time there were 8 sets of patients delivered, and the same bed occupied on 8 occasions; the third—an innupta, took place 26th of June, the interval being 3½ months, during which period there were 9 sets of patients delivered in the ward, and the same bed occupied on 7

occasions—was a case of bronchitis, under which she had been suffering for some months, peritonitis supervened; the fourth death occurred on the 5th July, an innupta of peritonitis, extreme despondency.

In No. 3 ward, 121 deliveries took place, out of which there were 4 deaths, 3 being zymotic; the first occurred the 16th December, was a case of phlebitis on admission; the second took place 25th March, a case of pyæmia, innupta, great distress of mind, after an interval of 3 months and 9 days, during which time 8 sets of patients were delivered, and on 4 occasions the same bed was occupied; and the third, a case of phlebitis, took place on the 14th June.

In No. 4 ward, there were 129 deliveries, and 4 deaths, 3 being of zymotic type, the first in November 30th, a case of peritonitis, had been in labour under the care of a woman for 2 days before admission; the second, 19th December, a case of peritonitis, innupta; the third, a case of hæmoptysis with peritonitis, on admission, 12th July, after an interval of upwards of six months, during which 17 sets of patients were delivered, and on 15 occasions the same bed was occupied.

In No. 5 ward, there were 122 deliveries, and 4 deaths, 2 of which were of zymotic type; the first an innupta, fretting greatly, occurred in December, sloughing of vagina and peritonitis ensued; in the interval there were 3 sets of patients confined in the ward, all of which went out well; the second, an innupta, fretting, occurred in March, attacked with mania and peritonæal symptoms on the 5th day; in the interval 5 sets of patients were delivered and made good recoveries.

In No. 6 ward, 124 deliveries took place, out of which there were 5 deaths, 2 of which were of zymotic character; the first occurred in November, 1870, was a case of peritonitis; the second took place in July, was also a case of peritonitis after an interval of 8 months, during which period there were 19 sets of patients delivered in the ward, and on 15 occasions the same bed was occupied.

In No. 7 ward there were 139 deliveries, out of which there were 5 deaths, 2 of which were of a zymotic type—the first in April, peritonitis, and the second in August, pyæmia, interval of 4 months, during which period 9 sets of patients were confined in the ward, and on 6 occasions the same bed was occupied.

In No. 8 ward there were 140 deliveries, and 3 deaths, 2 of which were zymotic—the first in January, the second in October, interval of 9 months, during which period 21 sets of patients were confined in the ward, and on 20 occasions the same bed was occupied.

In No. 12 ward, 140 deliveries, out of which 2 deaths, 1 of which was zymotic, an innupta.

I will now submit to your observation the following table,
No. 3:—

TABLE NO. 3.—Cases in which either Puerperal Fever, Peritonitis, Pyæmia, Phlebitis, Erysipelas, Scarlatina, Typhus and Typhoid Fever, or Variola occurred.

Date of delivery	No of		No. of Preg-nancy		Duration of Labour		Presentation		Mode of Delivery		Time of Attack. Hours after Delivery	Result to Mother		
	Ward	Bed	1st	Subt	1st Stage	2nd Stage	Natural	Pr. Natural	Natural	Instru-mental		Recovered	Died	
1870														
Nov. 5	6	68	—	3	7	3	N.	—	N.	—	24	1	—	Leg greatly swollen and in-flamed from erysipelas.
" 9	1	2	—	2	7	3	N.	—	N.	—	On ad-mission	—	1	Diarrhœa, feverish on ad-mission, low typhoid, and dysentery.
" 10	2	17	1	—	15	2	N.	—	N.	—	60	—	1	Bronchitis, pneumonia, diar-rhœa, peritonitis.
" 16	6	70	1	—	6½	1½	N.	—	N.	—	36	—	1	Great mental distress—sepa-rated from her husband; peritonitis.
" 20	12	113	1	—	5	1½	N.	—	N.	—	90	—	1	Innupta; strangled her child—remorse; perito-nitis.
" 21	1	3	1	—	10	2	N.	—	N.	—	70	1	—	Innupta; peritonitis.
" 24	3	27	1	—	10	2	N.	—	N.	—	On ad-mission	1	—	Confined at 6 a.m.; typhus; maculæ perceived at 9½ a.m. Sent to the Hard-wicke Fever Hospital.
" 24	4	39	—	5	9	8	N.	—	—	F.	48	—	1	In labour for two days before admission, under care of a woman; peritonitis.
" 26	5	48	1	—	14	10	N.	—	—	F.	68	1	—	Innupta; labour difficult; placenta retained from morbid adhesions; peri-tonitis.
" 27	6	67	—	2	14½	1½	—	Breech	N.	—	24	1	—	Innupta; in delicate health; 8th month, from fretting; child d. p.; peritonitis.
Dec. 8	3	24	—	2	12	2	N.	—	N.	—	On ad-mission	—	1	Inflamed varicose veins of leg and thigh; phlebitis.
" 10	4	42	1	—	4	1	N.	—	N.	—	70	—	1	Innupta; bronchitis on ad-mission; peritonitis.
" 21	5	44	1	—	22	6	N.	—	—	F.	24	—	1	Innupta; bronchitis; fret-ting greatly; sloughing of vagina and peritonitis.
1871														
Jan. 7	7	56	—	2	5	1	N.	—	N.	—	24	1	—	In delicate health from beginning of pregnancy; peritonitis.
" 8	8	84	1	—	—	8	N.	—	N.	—	6	1	—	Innupta; child born three hours before admission; peritonitis.
" 16	5	48	—	3	3½	2	—	Foot	N.	—	On ad-mission	1	—	Innupta; head and face enormously swollen from a beating; erysipelas.
" 19	8	85	1	—	11	9	N.	—	—	F.	50	—	1	Innupta; low, desponding spirits; sloughing; peri-tonitis. See Forceps Cases, No. 2.
Feb. 19	3	27	1	—	9½	½	N.	—	N.	—	26	1	—	Innupta; peritonitis.
" 24	7	60	—	6	21	2¾	N.	—	—	Per.	120	1	—	Difficult labour from deform-ity; inflamed varicose veins; phlebitis.
" 25	8	85	—	2	5	¼	N.	—	N.	—	24	1	—	Innupta; cuts on face; bruises over her body; has been drinking for last four months; peritonitis.
Mar. 3	5	44	1	—	9	¼	N.	—	N.	—	36	—	1	Innupta; fretting; mania on second day; peritonitis.
" 1	2	16	—	2	—	3	N.	—	N.	—	70	—	1	Innupta; roseolar rash on third day; sank rapidly; scarlatina.
" 6	8	86	—	4	6	¼	N.	—	N.	—	72	1	—	Feverish symptoms, with rash on third day. Sent to Hardwicke Hospital; scarlatina.
" 19	3	22	—	4	11½	½	N.	—	N.	—	48	—	1	Great distress from penury and anxiety of mind, hus-band having died; pyæmia.
" 26	7	60	1	—	4	2	N.	—	N.	—	18	—	1	In low spirits from begin-ning of pregnancy; pre-sentiment that she would die; peritonitis.

TABLE No. 3—Continued.

Date of Delivery	No. of		No of Preg-nancy		Duration of Labour		Presentation		Mode of Delivery		Time of Attack, Hours after Delivery	Result to Mother		
	Ward	Bed	1st	Subt	1st Stage	2nd Stage	Natural	Pr. Natural	Natural	Instru-mental		Recovered	Died	
1871 Apr. 8	8	87	—	2	3	$\frac{3}{4}$	N.	—	N.	—	96	—	1	Rash on third day; sent to Hardwicke Hospital; found that all her family had scarlatina.
" 9	8	85	1	—	7 $\frac{1}{2}$	$\frac{1}{2}$	N.	—	N.	—	72	1	—	Innupta; jaundice for last fortnight; peritonitis.
" 14	2	18	—	3	5	$\frac{1}{4}$	N.	—	N.	—	100	1	—	Bronchitis, with laryngitis, on admission; eruption on third day of scarlatina; sent to Hardwicke Hospital.
" 22	12	115	1	—	61	—	N.	—	—	Per.	20	1	—	Deformity of pelvis; very delicate, diminutive woman; angular curvature of spine; peritonitis.
" 25	2	20	1	—	12	$\frac{1}{4}$	N.	—	N.	—	On admission.	1	—	Labour brought on by being knocked down by a car; child d. p.; peritonitis.
" 26	4	41	1	—	10	6	N.	—	—	F.	12	1	—	Had been in hospital for three weeks in fever before admission; peritonitis.
May 5	1	2	1	—	38	2 $\frac{3}{4}$	N.	—	—	F.	12	—	1	In delicate health for three weeks; sloughing and pyæmia on delivery.
" 13	8	86	—	7	3	1	N.	—	N.	—	On delivery	1	—	Bronchitis for four months previous; peritonitis.
" 22	7	60	1	—	28	—	N.	—	—	F.	16	1	—	Innupta; aged 39 years; fretting greatly; peritonitis.
June 5	3	30	—	4	—	2 $\frac{1}{2}$	N.	—	N.	—	96	—	1	Nervous, delicate; mania on fourth day; saphena vein inflamed on the eighth day; died on ninth of phlebitis.
" 10	3	25	1	—	3 $\frac{1}{4}$	1 $\frac{3}{4}$	N.	—	N.	—	150	1	—	Innupta; fretting greatly; feverish symptoms; typhoid.
" 18	12	116	1	—	6 $\frac{3}{4}$	2 $\frac{1}{4}$	N.	—	N.	—	144	1	—	Innupta; fretting; peritonitis.
" 19	2	18	—	—	11	4	N.	—	—	F.	72	—	1	Innupta; bronchitis for last three months; fretting; want, penury; peritonitis.
" 25	6	69	—	5	3 $\frac{3}{4}$	1 $\frac{1}{4}$	N.	—	N.	—	36	—	1	Ill-treatment; over-exertion; fretting; bad husband; peritonitis.
July 2	2	19	1	—	31	5	N.	—	—	F.	40	—	1	Innupta; peritonitis.
" 5	4	42	—	2	2 $\frac{1}{2}$	$\frac{1}{2}$	N.	—	N.	—	On admission	—	1	Great abdominal tenderness, with hæmoptysis on admission; peritonitis.
" 9	8	84	1	—	30	6	N.	—	—	F.	48	1	—	Innupta; fretting; peritonitis.
" 16	3	24	1	—	7	3	N.	—	N.	—	24	1	—	Innupta; second day feverish; tympany, tenderness; peritonitis.
" 22	7	60	1	—	17	3	N.	—	N.	—	24	—	1	Innupta; sloughing of vagina; pyæmia.
" 23	8	85	—	2	2 $\frac{3}{4}$	$\frac{1}{2}$	N.	—	N.	—	76	1	—	Eruption appeared on third day; sent to Hardwicke Hospital; scarlatina.
Sep. 5	7	58	1	—	10	2	N.	—	N.	—	36	1	—	Fretting; bad husband; maniacal on fifth day; peritonitis.
Oct. 1	8	80	—	3	3	4	N.	—	—	F.	9th day	—	—	Delicate woman; bands in vagina; bad news on ninth day; erysipelas.
" 5	3	27	1	—	9	8	N.	—	—	F.	15	1	1	Innupta; fretting; feverish symptoms on third day; puerperal fever.
" 17	4	43	1	—	35	1	N.	—	N.	—	20	1	—	Child dead and putrid, result of a fall and blow; pyæmia, phlebitis.
" 23	8	84	—	4	—	6	N.	—	N.	—	24	1	—	Premature, fifth month; eruption appeared morning after; variola.

It sets forth the total number of zymotic cases, including puerperal fever, peritonitis, pyæmia, phlebitis, scarlatina, typhus and typhoid fever, erysipelas, and variola, which occurred in the hospital during the past twelve months. It is arranged in the order in which the several cases appeared, so that it will be seen that, although these sicknesses occasionally showed themselves, they were not the result of contagion or any endemic or epidemic influence in the hospital, but were simply owing to the circumstances in which each individual patient laboured either on admission or during her puerperal period.

On looking over the list it will be seen that 50 cases of zymotic disease, in one form or other, occurred.

Of these 50 cases, you will perceive that 23 were innupta, or nearly one-half; besides which 3 were labouring under great distress of mind, either from desertion by husband, want and penury, widowhood, or from *presentiment*; 3 were attributable to ill treatment, 2 to accident, being knocked down, one by a car, the other a blow; 3 had been in very delicate health for some time previous to admission, 1 had just left a fever hospital, 2 had acute bronchitis, and 1 had hæmoptysis, with peritonitis, on admission; 1 had typhus fever and 1 typhoid, 3 had erysipelas, 5 scarlatina, 1 variola, 1 patient was two days in labour before admission, and 1 the child was born three hours before she came to hospital.

As it is not essentially necessary, for the object in view, and as it would occupy too much of the valuable time of the Society to enter into the details of each case, I will merely mention that our treatment in peritonitis or puerperal fever consisted, for the most part, in turpentine fomentations and linseed-meal poultices to the abdomen. Turpentine and opium by the mouth, or by enema, where the stomach was irritable, supporting the strength with chicken-broth, beef-tea, wine or brandy.

In pyæmic cases, we found the tincture of the muriate of iron, with chlorate of potash, of service.

In cases of phlebitis, poulticing or applying powdered starch, cotton wadding, and oiled-silk, or gutta-percha cloth, over the part affected, and treating them constitutionally with bark and chlorate of potash and anodynes—supporting the strength at the same time with nutritious diet and wine—was found beneficial.

And, again, in order still further to prove that, although diseases of a contagious nature may prevail outside, yet the hospital be perfectly exempt from it, I refer you to the table No. 4, framed, as in my former reports, in accordance with that of the Registrar-General, by which you will perceive that from No. 1 North City District 291 patients were admitted and delivered. Of these there were 5 deaths from zymotic complaints; while we find that 241 deaths of the same type occurred in the district within the year.

From No. 2 North City District 256 women were admitted and delivered. Of these there were 6 deaths from zymotic complaints; while there were 169 deaths of the same type in the district in the same period. From No. 3 North City District 145 women were delivered in the hospital, 3 of whom died from zymotic complaints; while there were 266 deaths of the same nature in the district during the period. From No. 1 South City District 41 patients were delivered; no deaths occurred, yet we find that 280 deaths from zymotic diseases took place in the district within the year. From No. 2 South City District 118 patients were delivered, of which 1 died from a zymotic complaint; at the same time we find 134 deaths from zymotic diseases occurred in the district during the year. From No. 3 South City District 133 women were delivered. Of these there were 4 deaths from zymotic diseases; while we find that 251 deaths of the same nature took place in the district in the same period. From No. 4 South City District 177 women were admitted and delivered, out of which there were 2 deaths of zymotic complaint; there were 352 deaths occurred from zymotic diseases in the district within the year.

Thus showing that, although there may be an enormous amount of fatal disease occurring outside, the hospital proper may remain, comparatively speaking, free from it, and is a strong corroborative proof, if any further was wanting, that a large maternity can be a safe asylum for parturient women.

FORCEPS DELIVERIES.

There were 64 cases where delivery had to be effected by the forceps, 47 of which were primiparæ, and 17 pluriparæ, giving birth to 26 male children, 2 of which were dead born; one $8\frac{3}{4}$ lbs. weight, the mother having been 11 hours in the second stage, owing to the head being in the second position; the other weighed 9 lbs. 6 oz., the mother had been in labour for 2 days before admission; both mothers recovered. All the female children, 22, were born alive. Ten children died sometime after delivery, viz., 7 boys and 3 girls, 6 of which died from exhaustion or compression, viz., 4 boys and 2 girls; 2 boys died of erysipelas of head; 1 of convulsions, a boy; and 1, a girl, was overlain. Fifty-five of the mothers recovered. Of the 9 deaths, 6 were primiparæ, 5 of which were innupta, and 1 doubtful, and are thus accounted for.

DEATHS IN FORCEPS CASES.

The first—fretting greatly; 28 hours in labour, 6 of which were in the second stage; head arrested at the brim, owing to inertia and disproportion. Long forceps; peritonitis appeared in 24 hours; sloughing followed, and she died on the fifth day.

The second—great despondency; had been drinking from a pint to a quart of whiskey per diem for the last seven months; 9 hours in the

second stage, owing to inertia; peritonitis set in within 30 hours, followed by sloughing, and she died on the morning of the third day.

The third—her state was doubtful; in very low spirits; states that “her husband is in America.” She was 6 hours in the second stage from inertia and disproportion. She died suddenly on the second day. *Post-mortem* examination—nothing abdominal discovered beyond a clot in the left ventricle of the heart.

The fourth—in very delicate health for the last three weeks; labour slow in both stages (40 hours). Antimonial solution, warm bath, and opiate had to be given in first stage. Stimulating enemata and ergot in the second without avail. Great swelling and inflammation of the soft parts (owing to too frequent examination), followed by sloughing and great irritative fever. Pyæmia appeared on shoulders, elbow, and hand on the third day, and she died on the ninth day.

The fifth—great distress of mind; labour of 36 hours’ duration, 5 of which occupied the second stage. In the first she had to be put in a warm bath, after which an opiate was given. When the os was fully dilated, two stimulating enemata were administered, but not producing any effect, the head, which was arrested at the brim, making no advance, owing to inertia and disproportion. The long forceps was applied, and after some difficulty a girl (living, 8 lbs. 6 oz.) was extracted. She went on favourably till the second day, when she got an attack of diarrhœa, followed in the evening by peritoneal symptoms, and she sank rapidly the day after.

The sixth—had been suffering, and under treatment for “dropsy” for some time previous to admission; was weak and fretting greatly; her labour was slow from inertia. The usual treatment failing, chloroform was administered, and she was delivered, with the forceps, of a boy (living, 8 lbs. in weight). There was nothing remarkable in her symptoms the following day, but on the morning after she was found in a state of collapse, and she died in a few hours. *Post-mortem* examination—discovered a small rent in the anterior wall of cervix uteri, which was quite in a soft and pulpy state.

Of the three pluriparæ:—

The first—fifth pregnancy; brought in in the second stage of labour, having been under the care of a nurse for two days; ascertained that she had been in the second stage for at least 6 hours; head found in the cavity, with large caput succedaneum. Two stimulating enemata failing, and as the pulse was rising, forceps were applied, and delivered of a girl (living, 6½ lbs). Symptoms of peritonitis showed themselves on the second day; the usual treatment was adopted. On the third day discovered that the rectum and colon were loaded with hardened fæces, so much so as to prevent the passage of the long tube. Every means were tried to overcome the obstruction, by stupes, croton oil, &c.; vomiting

supervened, and she died on the sixth day from peritonitis, evidently owing to the obstruction. No *post-mortem* could be procured.

The second—second pregnancy; in great distress of mind; bronchitis for last two months; labour slow in consequence of narrowing of pelvic arch; was delivered with some difficulty. Cough continued, together with low desponding spirits; peritoneal symptoms appeared on the third day, and she sank on the seventh under great mental suffering.

The third—third pregnancy; brought in by a medical man, under whose care she had been in labour for some hours previous. The head was found in the cavity, without having made any advance for three hours; bands also were discovered in the vagina, the result of difficult labour in her former confinement, besides which there was flattening of the right horizontal ramus of the ileum and pubis; pains strong. She was put under chloroform, and delivery effected, after some difficulty, with the forceps. Some sloughing of the vagina supervened; however, by the injection of an antiseptic solution and charcoal poultice, it improved—so much so as to be able to be up on the eighth day, intending to go out on the following, but some unpleasant intelligence having been conveyed to her, feverish symptoms set in, erysipelatous blush appeared over the nates and down the thighs, from which she sank on the fifth day after the attack.

CRANIOTOMY.

Craniotomy had to be performed in six instances, two primiparæ, and four pluriparæ. The first, aged 19, first pregnancy.—Admitted with great dyspnœa, suffering from bronchitis for last five weeks. Brow found presenting at the brim, and not making any advance for four hours the pulse became rapid; the long forceps were applied, but ineffectually; the head was lessened, and delivery effected by the crêchet. The pulmonary symptoms could not be alleviated, and she died on the third day of the bronchitis.

The second, aged 30, fourth pregnancy, was a case of accidental hæmorrhage at full term, without any assignable cause; the membranes were ruptured; and a binder, with pad, was tightly adjusted; the head descended on the brim, where it was arrested. Ergot was administered without any effect; she was put under chloroform, and the long forceps applied, but it was impossible to extract the head in consequence of an exostosis on the horizontal ramus of left pubis; obliged to perforate and deliver with the cephalotribe. She got an attack of phlebitis, but eventually recovered.

Third, aged 19, first pregnancy.—A small, diminutive creature, with angular curvature of spine, pelvis under size. Long forceps tried, but all efforts to draw down the head failing, the head was lessened, and delivery effected, after great difficulty, with the cephalotribe. She had an attack of peritonitis, with intense sickness of stomach and great

prostration, requiring champagne, all the other stimulants disagreeing. She continued for upwards of ten days in a state of great danger, but eventually recovered.

The fourth, aged 30, fourth pregnancy. Her two former deliveries had to be effected by craniotomy. On examination there was found great narrowing of the upper strait. As soon as the os was sufficiently dilated she was put under chloroform; the hand was introduced, the membranes ruptured, and version performed, but found it impossible to bring down the head, even with the forceps; funis having ceased to pulsate for some time, the head was lessened, and delivered with the crotchet, assisted by the forceps. She made a good recovery.

The fifth, aged 30, fourth pregnancy. A case where prolapse of funis occurred; the head being at the brim, the long forceps were applied, but proved ineffectual in consequence of considerable diminution of ant. post. diam. ; the funis ceasing to pulsate, the head was lessened, and crotchet failing, had to apply the cephalotribe, and after great difficulty delivery was effected. She recovered.

The sixth, aged 33, third pregnancy, was a footling presentation; found it impossible to deliver by the ordinary means in consequence of great projection of the promontory of the sacrum; the funis having ceased to pulsate for some time, the head was lessened, and delivery effected by the crotchet. She made a good recovery.

VERSION.

There were 7 cases where we were obliged to perform version, 5 of which were owing to the upper extremity presenting; in one, a case of deformity, where we were eventually obliged to deliver by craniotomy, and one, a case of prolapse of funis, where the waters had escaped eight hours previous to operation. Five of the children were born alive; of the two dead, one had to be delivered by craniotomy; in the other instance the liquor amnii had drained away four days prior to admission. All the mothers recovered.

PROLAPSE OF FUNIS.

There were 8 instances of prolapse of the funis: 1 a primiparæ, 7 pluriparæ. In 2 delivery was effected by version, both children alive; in 2 the funis was pulseless on admission, so labour was allowed to be terminated by the natural efforts; in 2 others the labour was so quick as not to require interference; in both the child was born alive, but one survived only ten minutes; one was delivered by the forceps, child lived only a few minutes; and in another instance the forceps were tried, but owing to contraction of the brim the head had to be lessened, funis having ceased to pulsate. All the mothers recovered.

PLACENTA PRÆVIA.

There was one case of unavoidable hæmorrhage, aged 40, tenth pregnancy, admitted in the eighth month, in a state of great exhaustion, hæmorrhage having set in a week before. She was plugged, strength supported, and as soon as the os would admit, the hand was introduced and version^a performed, and the child, d. p., extracted. Every means tried to arrest the draining, which continued; beef-tea and brandy, both by mouth and enema were given, but she sank in eight hours after delivery.

ACCIDENTAL HÆMORRHAGE.

There were eight cases of accidental hæmorrhage, all being pluriparæ, viz., one her third pregnancy, one her fourth, one her fifth, two in their sixth, two in their eighth, and one in her eleventh pregnancy, giving birth to six male children, two of which were alive; two females, both of which were alive. All the mothers recovered.

POST-PARTUM HÆMORRHAGE

occurred in 5 instances, 4 in primipara, 1 in her second pregnancy, 1 a primipara, died of gangrene of the uterus; all the others recovered. In three cases the ergot and cold water by injection was sufficient to arrest the hæmorrhage; in 2, had to employ the solution of perchloride of iron.

PLACENTA MORBIDLY ADHERENT.

There were nine cases where placenta was retained from morbid adhesion, 3 in primiparæ, and 6 in pluriparous patients; 8 recovered; 1, a primipara, died from rupture of a varicocele in the left spermatic vein.

CONVULSIONS.

There were two cases of eclampsy, both primipara, one a case of twins; the first was attacked with convulsions when the os was nearly fully dilated, cold effusions, turpentine, and assafoetida enema, sinapisms to calves of legs and back of neck, and delivery was effected by the forceps, of a boy living. She was afterwards put on bromide of potass, with belladonna, with good effect, although having an attack of mania on the fourth day, she eventually went out well. The second twin case, the convulsions did not occur till 7 hours after delivery. She was treated in the usual way with turpentine, assafoetida enema, sinapisms, cold effusion, and afterwards chloroform, with good effect. Her convalescence progressed slowly but favourably, and was discharged well. This woman had altogether 20 fits.

* The operation of version having been performed on the 5th November, 1870, is not included in the version cases of this report, but the death not taking place till the morning of the 6th, has now to be recorded.

MANIA.

Mania occurred in 12 instances, 7 of which were primiparæ, and 5 pluriparæ; all were delivered by the natural efforts; 2 of the primiparæ had to be sent to the lunatic asylum; 6 were discharged sufficiently recovered; 2 their husbands removed, 1 on the eighth day, and 1 on the seventh day. There were 2 deaths, one an innupta; mania took place on the second day, followed by peritonitis, of which she died; one, a delicate, irritable woman, received some bad news on the fourth day, which brought on the attack; phlebitis ensued, of which she died.

The treatment generally adopted was, in the more severe cases, by solution of tartar emetic, and afterwards the hydrate of chloral; in those of a less severe character, we found the chloral or opium was quite sufficient.

I have now merely to say, that feeling fully convinced of the inestimable value of statistics, derived from such sources as these have been, where their truthful accuracy can really be depended upon, I have been induced to bring them forward, without any comment of my own, and simply lay them before you, in order to show the real sanitary condition of a large maternity, so that you may be able to draw a just and fair conclusion as to the safety, or otherwise, of such institutions as the Rotundo Lying-in Hospital.

Dr. CHURCHILL, whose opening remarks were inaudible, spoke as follows:—I think Dr. Johnston has proved that the theory set forward that the patient is more likely to contract disease in an hospital after confinement than if delivered at her own home, has little or no foundation. For my part I must say that the Obstetrical Society is under very great obligations to Dr. Johnston, and I hope it will not be the last contribution we shall receive from him on the subject.

Dr. ATTHILL said—I think, sir, that all who take an interest in an institution like the Lying-in Hospital, the first hospital of the kind in Great Britain, cannot but feel deeply grateful to Dr. Johnson for the trouble he has taken in framing the report which he has laid before us. It is, indeed, impossible to over-estimate the value of the details which he has drawn up with so much care. I think Dr. Johnston has clearly proved that the mere fact of a large number of puerperal women being delivered and congregated together in one building does not necessarily generate disease. There is one point I wish to refer to. Stress has been laid by some writers, who take a view adverse to lying-in hospitals, on the fact that the *relative* mortality of the Rotundo Hospital has largely increased. It seems to me, however, that this is capable of easy explanation, if it be borne in mind that the Lying-in Hospital was formerly extensively resorted to by the wives of the artisan class, who were in the

habit of going into it to be confined there, but who now, from various causes, do not seek admission, and who, except in comparatively few instances, are now delivered at home. But the destitute, who are peculiarly prone to disease, the wretched victims of seduction, and the bad cases—sent in because they are bad—are as numerous as ever, and it is among these that the mortality, as shown by Dr. Johnston's report, mainly arises. The mortality of some other hospitals has been favourably, although most unfairly, contrasted with the Rotundo, for those hospitals do not admit any except married women; and I think it should be borne in mind that the number of unfortunate women who represent themselves as being married on seeking admission, the "bad cases," and the wretchedly poor, are as numerous as ever, while the number of respectable women who seek admission has fallen off.

Dr. MCCLINTOCK said—Dr. Johnston's paper is one of a series of reports which have been submitted to us from time to time, and are of the very highest value as bearing on the subject of hospitalism, about which so much has been written. He has proved that most of the deaths are not fairly attributable to hospital air or circumstances; that, in fact, the patients would have met their deaths anywhere else as certainly as in the wards of the Rotundo. I think this report is a most important step towards the settling of this vexed question of hospitalism. There are two sources of mortality in the Rotundo Hospital which I have at different times before alluded to. One large source of mortality arises from the number of unmarried women who go into the hospital, and there is no class of persons among whom the proportion of deaths is so great in childbed. Dr. Johnston has shown what a large number of those unfortunate women took refuge in the hospital. That is one important fact, and the other is the immense number of primiparous cases. Out of 1,161 cases admitted I find there were 553 primipara, nearly half the entire number, and there is a much greater danger to life amongst this class of cases than any others, as every man of experience well knows. Dr. Duncan has shown that the number of primiparous cases is about 23 per cent. of all cases in the population at large, but we have double that number in the Lying-in Hospital during the year of the present report. This is a very important fact, and should be put conspicuously forward. I thought it well, sir, to allude to these two statistical features of the report, as I think them of great importance. It is apparent that the relative number of primiparous cases has been increasing of recent years in the Rotundo. During Dr. Collins' Mastership they were 30½ per cent. of the entire; during my own seven years' Mastership they were 37 per cent.; and in this present report, drawn up by Dr. Johnston, they are about 47 per cent.

Dr. DENHAM said—There are one or two subjects I would wish to

allude to. Dr. Johnston has given us the number of persons treated outside the hospital, but he has not given us any comparison between the mortality in the hospital and that which occurred outside. I am not aware if Dr. Johnston is able to furnish the information, but I think it would be a satisfactory thing to know the amount of mortality outside as well as inside the hospital. As to the mortality* among primiparæ, that is a thing in which we are all concerned. With regard to the number of married women, we have no certain data to show that a woman admitted into the hospital is married, and it is probably only at the hour of death that it comes to the knowledge of the Master or the Assistant that she is not married; and I am sure that a number of women have come in thus, and have had a very narrow escape of their lives, have gone out leaving the impression that they were married women. Of course on admission we have no knowledge of this. We must also remember that the mortality in this hospital cannot bear an equal proportion with that of any other. When a girl comes up from the country and flies away from her friends, and perhaps gets into some miserable lodging, she, after remaining there for a considerable time in a state of semi-starvation, falls into bad company, and then comes into the hospital for her confinement; it is very important to bear these facts in mind.

Dr. FITZPATRICK said—There is one portion of Dr. Johnston's paper with regard to forceps cases to which I would wish to refer. I remember some twenty years ago, though not occupying any very important position, I was taught not to use the forceps except in extreme cases, but now the forceps are frequently used. I think there is one thing that should be impressed on the present generation of students, that is, that the use of the forceps may be pushed too far, just as the crochet was used in former times improperly, and while I am quite sure that the present Master of the Lying-in Hospital would be certain to use the forceps judiciously, perhaps those he teaches might not do the same; I therefore feel that the great danger may be that the forceps may be used when they are not required at all. I therefore hope that students will be taught not to introduce the forceps unnecessarily. I feel this is a subject that requires a great deal of consideration from the teachers of the present day, for it would be a great evil if we were to rush into too frequent a use of the instrument.

Dr. JOHNSTON said—I thank very much the gentlemen who have spoken so flatteringly of my report. There is one thing that has been alluded to, namely, my not having made any comment on the report, but my idea was to lay the actual facts before the meeting, and to allow the members to make any commentaries on it they pleased; in a word, to allow you to draw your own conclusions. With regard to Dr. Denham's observations, I have confined myself entirely to the internal cases, as my

object at present is altogether with regard to the hospital proper, and besides which I have not the means of forming an accurate report of the extern cases, not being able to get the particulars from the lips of the patients themselves, and that, I had no means of doing, and there is no use in bringing forward a report unless its details can be critically examined into. Without wishing to make any boast I may state that for the purpose of securing a perfectly correct report I have taken the trouble of entering the particulars of every case myself, not from any book or other report, but from the lips of the patients themselves, so that the returns I have furnished are, so far as lay in my power, accurate. With regard to the forceps, I have heard all that Dr. Fitzpatrick has said, and I can assure him we always make it a point to impress upon our pupils the risk attending the use of the instrument; although in the hands of a practised physician they are so beneficial, we explain to them the many serious results arising from its being used by unpractised hands. We try to let nature effect its purpose, but when we find that she is unable we do not hesitate to call in the forceps to our assistance.

TRANSACTIONS OF THE CORK PATHOLOGICAL AND MEDICO-CHIRURGICAL SOCIETY.

Puerperal Convulsions.—The following case, reported by my friend and former pupil, Mr., now Dr. Lindsay, presents no very remarkable features, but taken in connexion with the interesting cases of puerperal convulsions read by Dr. E. Townsend, jun., may be of interest.

Anne Edgehill, aged twenty-two, primipara, naturally robust, and of healthy constitution, and remarkably active in her habits; when at the full period of utero-gestation, over-walked herself, and had besides some annoyance from her domestic arrangements. She had noticed for some days previous to her labour, that the face, submaxillary regions and hands presented an œdematous appearance.

On the 10th of May, 1866, complained of headache, and slight labour pains, followed by a severe convulsion, during which the tongue was much lacerated. Shortly after the arrival of the midwife a second similar attack occurred, and a third after the arrival of Dr. Cummins, who gave her a dose containing six grains of calomel and two drops of croton oil.

Although fully conscious between the paroxysms, it was noticed that the labour pains were attended by twitching of the right arm, spasm, and grinding of the teeth; her skin was moderately warm, and vomiting occurred from time to time. As the breathing soon became stertorous, Dr. Cummins thought it advisable to bleed her, and drew off sixteen ounces of blood, after which the pulse became full and soft, the breathing

more regular and natural, and both stertor and spasms ceased—pains infrequent and not severe. The urine drawn off contained a high percentage of albumen and had a smoky appearance; it was also scanty.

May 11th.—The os uteri, which on the first examination was about the size of half-a-crown, dilated with great rapidity after the bleeding, so that at 2.40 a.m. the foetal head had passed through, and reached the perineum. Dr. Cummins now fearing a recurrence of the convulsions, delivered a healthy living child with forceps, when almost complete relief followed, the pupils contracting readily on exposure to light, and the mind and body in a state of repose. At 3.20 a.m. a convulsion occurred of lighter description than any of the preceding, and of about five minutes' duration.

Dr. C. visited patient again at noon, and was informed that she had had frequent and severe returns of the fits. The skin was hot; urine scanty, and containing much blood. Patient semi-conscious; pulse 100. The head was now shaved, and a blister applied to the occiput, and as the bowels had not acted, the bolus of calomel and croton oil was given as before.

3.30 p.m. Has had two severe paroxysms since, and is quite unconscious; bowels have not acted; turpentine stupes ordered to the abdomen, and a repetition of the calomel and croton oil.

8.30. Has had only one slight paroxysm since. Swelling of face and hands has decreased very much. She has urinated freely without catheter, and is now sleeping tranquilly. Pulse 95. The medicine has not operated.

May 12th.—No convulsion since, but is unable to recognize Dr. Cummins; denies that she is married, and will not believe that the infant is her's. Bowels have been moved, and kidneys have acted freely. Pulse down to 70; mammæ enlarging. Turpentine fomentations continued.

May 13th.—Spent a good night; no appearance of convulsions or of lochial discharge. Bowels have acted again. Pulse 80. Tongue much ulcerated. Is only partially conscious, not knowing the doctor, and believing him the clergyman. Abdomen to be still fomented, a blister applied behind each ear, and to get a dose of castor oil.

May 14th.—Spent a good night; knows the doctor perfectly, and is quite rational in every respect; pulse 60; urine copious and containing albumen.

From this date the patient steadily improved, and in a few days, being quite restored to health, and urine normal, she was allowed to get up and resume her usual avocations. She has been twice confined since without convulsions.

There is only one remark I would make in connexion with this case, viz., that the benefit which followed bleeding, both in its effect upon the

os uteri, or the nervous system, illustrates, in common with many which have come under my observation, the fallacy of a statement which is made in the last edition of one of the best class books in the heads of students, that bleeding is to be deprecated on *all grounds* in this disease.—*January 9, 1871.*

Case of Acute Spontaneous Gangrene from Arteritis.—By FRANCIS M. LUTHER, M.D.

On the 22nd of February, I was called to see John Neale, labourer, who complained of agonizing pain in his right leg, from the knee down. When I arrived his friends were chafing it vigorously. There was nothing peculiar in the appearance of the limb, except that the veins looked dark, as if the blood did not circulate. The skin was redder than natural, which I ascribed to the friction to which it had been subjected. The man told me that on the previous day he had been attacked with dreadful pains in *both* legs, that after a while all the pain concentrated itself in the right leg, which became white as tallow, or like the limb of a corpse.

I thought this exaggeration until the attending clergyman corroborated the man's statement, as to the singular appearance the leg had presented when he saw him.

My first impression was that the case was one of embolism, but reflection satisfied me it could not be that, as both limbs were first attacked, and no clot or growth detached from the heart would be likely to block up the aorta at its bifurcation. Phlebitis occurred to me, but then friction did not increase the pain, it rather relieved it, and in phlebitis pressure upon the inflamed veins cannot be borne. Still I would have supposed it a rheumatic attack of the muscles of the calf, such as often follows influenza. (The man had not long recovered from that malady. He went to work before he was well of it). But the description of the tallowy whiteness of the leg pointed to a sudden arrest of the circulation. I gave an emeto-cathartic, prescribed hyposulphite of soda, and directed tincture of iodine to be painted on the leg. Next day the man, who was very poor, expressed a wish to go to hospital. Never having met a case precisely similar, I wrote to Dr. O'Reilly about it, and intimated that I expected gangrene to supervene. A week after, the doctor told me that the limb from some inches below the knee was quite livid, and the cuticle raised in bullæ. He got opium in hospital—the leg was wrapped in cotton wadding. Afterwards disinfectants were used. Dr. O'Reilly considered that amputation above the knee was out of the question, as Neale was elderly and much emaciated. On the 15th March I was again summoned to visit Neale, who, now convinced that his case was nearly hopeless, came home to die. The leg was black all but the toes, which,

deprived of cuticle, looked almost of a natural colour. They were, however, of an icy coldness. The stench was overpowering. I used plenty of carbolic acid, fumigated the room with sulphur, exhibited hyposulphite of soda and morphia.

Having read of a case in the Paris Journal of Medicine which was treated by laying bare the bones and applying lint wet with nitric acid which made them friable—(the writer said that the stump was very fair)—on the 24th I cut down to and around the bones. No blood flowed except a few drops when clearing the inner side of the tibia. Neale said "*that hurts*," and begged me not to finish till next day. I agreed, and stuffed the horrid chasm with lint soaked in Condyl's fluid. On the 25th he told me his jaws were getting stiff, which he attributed to my having touched "*a vein*" with the knife. I feared the advent of lock-jaw, but thought it better to remove the limb with the saw at once. I did so, very rapidly having first swept the scalpel round the bones. Unfortunately, poor Neale, as also myself, was exposed to a bitter east wind on every occasion that I operated, as the window was not glazed, and the shutter had to be taken down to give light. Upon this day he was bathed in perspiration. I stuffed the stump with lint soaked in warm Condyl's lotion; gave whiskey and beef-tea. Some hours after the jaws were closing gradually, but he could still eat bread and broth and take punch. I exhibited Cannabis Indica. From his weak state I considered it useless to try heroic remedies. Next day he could just separate his teeth. When asked about it he found his neck stiff. He perspired profusely. There was no risus sardonicus, nor any convulsion. He suffered little. On the 28th he died. I regret I did not test the urine for sugar, or employ some antidotal remedy steadily to correct blood-poisoning. I doubt if the man took the hyposulphate. Dispensary patients are not very tractable. I think the friction was injurious, and the iodine painting equally so. Hot air baths followed by the warm douche might be of service. The cases are rare and require prompt recognition and prompt treatment. A girl comes to the dispensary on crutches who tells me, that seven years since her leg mortified in hospital, after a fever. Dr. Mangan sawed through the bones, leaving the tibia protruding. Doubtless he meant to remove this subsequently; but one day, in hopping across the ward, the girl fell and the bone snapped off high up in the stump, which then healed, and is as perfect as though made by the knife! The young woman has preserved the bone. Query: in cases of acute spontaneous gangrene not following upon any wasting illness, is amputation through the sound flesh above the articulation advisable?—*January 9th, 1871.*

On the Use of Pepsine Wine in the Artificial Feeding of Infants.

By W. JACKSON CUMMINS, M.D.

The value of pepsine in those forms of dyspepsia attended by deficient secretion of gastric juice, is so well known and generally understood, that it is unnecessary for me to trespass on the time of the Society by more than an allusion to them. In the diseases of children, however, and especially as a substitute for a wet nurse, when a mother is unable or unwilling to suckle her own child, the benefit of this valuable aid to digestion is not I believe as generally known, although allusions to it are to be found in medical essays.

Dr. West, in his "Diseases of Children" (5th edition), speaks of it in the following words:—"Recently I have employed Morson's pepsine wine in 10 or 15 minim doses three or four times a day with considerable advantage."—(Article on Dyspepsia, p. 591.)

Dr. Joulin has also written a paper in the *Brit. and For. Med. Chir. Review* of June, 1862, recommending "the employment of pepsine in all cases of congenital feebleness with arrest of development of the digestive system, and even in complicated cases in which the lesion affects at the same time the digestive and respiratory systems."—(Quoted from *Waring's Therapeutics*.)

M. Corvisart also, in the *Review Med. Chirurg. de Paris* of 1856, states that pepsine has proved efficacious in his hands in the diarrhoea of young infants.—(*Ibid.*)

There is nothing of course like a good breast of milk for an infant, if it can be had; and in the "good old times," when the peasantry and small farmers lived on potatoes and milk, without stimulating their nerves with strong tea, nor their brains with penny-a-liner's novels, there was an ample field for the selection of a foster parent, but now even where that *rara avis*, a good nurse, is procured, she is so independent and knows her power so well, that any caprice must be humoured, and she is always ready to throw up her situation or neglect her charge.

A wet nurse is, then, an admitted torment, and a balance struck between its advantage and disadvantage is generally against the former.

Artificial feeding by bottle is a great improvement upon the old system of spoon feeding, as the act of sucking stimulates the salivary glands and ensures due insalivation, which is an important part of infantile digestion. With such an aid the stomach of most human infants is vigorous enough to fall into the way of digesting cows' milk, properly diluted, and mixed with sugar and cream to assimilate the proportion of its constituents to human milk—but besides the relative excess of casein and albumen contained in cows' milk when compared with human, the coagulum of the latter is "soft, flocculent, and not so thoroughly

separated from the other elements of the fluid as the firm hard curd of cows' milk is from the whey in which it floats."—(West.)

And when we reflect that the digestive organs of the *human* infant are found to digest human milk and the force of its gastric juice proportioned to the solution of its soft flocculent coagulum, we can understand why the solvent power of its gastric juice is sometimes unequal to re-digesting the firm curd of cows' milk. When such is the case, acetous fomentation is quickly set up, offensive gasses distend the stomach and taint the breath, vomiting and diarrhoea set in, and in process of time the little patient sinks into a miserable state of marasmus, and dies.

The remedy for this state of things is simple, for although we cannot change the elementary composition of the milk we have to use, we can introduce into the infant's stomach a digestive power proportioned to the food it has to use—the organic principle of digestion taken from the stomach of the calf.

It is now many years since I first applied this simple theory to practice in the case of one of my own children, who, when about three or four months old, was reduced to a condition of marasmus by vomiting and diarrhoea, due to imperfect digestion of cows' milk. I ordered him 15 or 20 drops of pepsine wine, to be given immediately before or after each meal. Soon after commencing it he began to improve, and by degrees all bad symptoms vanished, and nutrition was quite restored. The pepsine was continued until he was nearly two years old, and he throve at least as well as if he had been wet nursed; other treatment of course preceded and accompanied the use of pepsine, but it was not until the latter was commenced that improvement took place.

Shortly after a child, born in England, and bottle-fed, was brought over to this country when about six months old; he also was suffering from infantile dyspepsia, and was pining away in a listless apathetic state, quite indifferent to surrounding objects, and appearing as if he would lapse into idiocy from mal-nutrition of the nervous centres.

I immediately ordered him pepsine wine, which produced such beneficial effects that after it had been continued about twelve months, he had become a bright, intelligent, well-nourished child.

Since then I have never recommended a wet nurse, and have used pepsine wine largely in dispensary, hospital, and private practice, and have seen many apparently hopeless cases recover under its use.—
February 6, 1871.

BOOKS RECEIVED, FEBRUARY, 1872.

1. The Medico-Chirurgical Transactions. Volume fifty-fourth. London : Longmans, Green, Reader, and Dyer. 1871.

2. On the treatment of fractures of the limbs. By Sampson Gamgee, Fellow of the Royal Society of Edinburgh, Surgeon to the Queen's Hospital, Birmingham. London. J. and A. Churchill. 1871. 8vo, pp. 296.

3. On the relation of therapeutics to modern physiology. By Henry R. Madden, M.D., Edin. London : H. Turner and Co. 1871. Sewed.

4. The principles and practice of surgery. By John Ashurst, jun., M.D., Surgeon to the Episcopal Hospital, Surgeon to the Children's Hospital, &c. Illustrated with five hundred and thirty-three engravings on wood. Philadelphia : Henry C. Lee. 1871. 8vo, pp. 1011.

5. The Birmingham Medical Review, for January, 1872. Birmingham : Cornish, Brothers.

6. The collated statistics of English surgery in public medical charities, for 1870. By S. Messenger Bradley, F.R.C.S., and Walter Whitehead, F.R.C.S. London : Simpkin, Marshall, and Co. 1861. Sewed.

7. A practical treatise on the diseases of the lungs, including the principles of physical diagnosis, and notes on climate. By Walter Hayle Walshe, M.D. Fourth edition, revised and much enlarged. London : James Walton. 1871. 8vo, pp. 667.

8. The Mining Magazine and Review, for January, 1872. London : Henry S. King and Co.

9. On chronic hypertrophy of the lips. By R. W. Taylor, M.D., Surgeon to the New York Dispensary. New York : William Baldwin and Co. Sewed.

10. Cancerous and other intra-thoracic growths, their natural history and diagnosis. By James Risdon Bennett, M.D., Fellow and Senior Censor Royal College of Physicians. London : J. and A. Churchill. 1872. 8vo, pp. 189.

11. Transactions of the American Ophthalmological Society. New York : D. Appleton and Co.

12. Practical lessons in the nature and treatment of the contagious diseases ; an account of the primary syphilitic poison, and of its communicability. By John Morgan, M.D., F.R.C.S., Surgeon to Mercer's Hospital and to the Westmoreland Lock Hospital, Professor of Surgical and Descriptive Anatomy Royal College of Surgeons, Ireland. Sixty-seven illus-

trations. London : Baillière, Tindall and Cox. 1872. 8vo, pp. 335.

13. Inaugural address, including a paper on infant asylums. By A. Jacobi, M.D., President of the Medical Society of the County of New York. New York : D. Appleton and Co. 1872. Sewed.

14. Bromide of sodium. By Meredith Clymer, M.D.

15. Diseases of hair ; a popular treatise upon the affections of the hair system, with advice upon the preservation and management of the hair. By Benjamin Godfrey, M.D., F.R.A.S. London : J. and A. Churchill. 1872. Fcap. 8vo, pp. 179.

16. Remarks on the prevalence and distribution of fever in Dublin ; illustrated by a map, tables, and diagrams, with appendices, on sanitary matters in that city. By Thomas W. Grimshaw, M.D., (Dub.), Fellow and Censor College of Physicians, Physician to Dr. Steevens' Hospital and to the Cork-street Fever Hospital. Dublin : Fannin and Co. 1872. Sewed.

17. Notes on syphilis, with an appendix on the unity of the syphilitic poison. By S. Messenger Bradley, F.R.C.S. London : J. and A. Churchill. 1872. 8vo, pp. 48.

18. On the mechanism of accommodation for near and distant vision. By R. G. Dudgeon, M.D.

19. A report of surgical cases treated in the army of the United States, from 1865 to 1871. Washington : Government Printing Office. 1871.

20. Approved plans and specifications for post hospitals. Surgeon-General's Office, Washington, July, 1871.

21. Psychic force and modern spiritualism ; a reply to the *Quarterly Review* and other critics. By William Crookes, F.R.S. London : Longmans, Green and Co. 1872. Sewed.

22. Nouveau dictionnaire de médecine et de chirurgie pratiques illustré de figures intercalées dans le texte. Tome Quatorzième Erys, Fl. Paris : J. B. Baillière et Fils. 1871. 8vo, pp. 779.

23. The power above matter : an address read before the Hunterian Society. By Dennis de Berdt Hovell, President of the Society. London : J. and A. Churchill. 1871. Sewed.

24. Recollections of past life. By Sir Henry Holland, Bart., M.D., F.R.S., D.C.L., President of the Royal Institution of Great Britain. London : Longmans, Green and Co. 1872. 8vo, pp. 346.

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GREAT BRITAIN.

1. The British and Foreign Medico-Chirurgical Review. Churchill.
2. The Edinburgh Medical Journal. Oliver and Boyd.
3. The Retrospect of Medicine. Edited by W. Braithwaite. Simpkin, Marshall, and Co.
4. The Half-yearly Abstract of the Medical Sciences. Churchill.
5. Pharmaceutical Journal. Churchill.
6. The Lancet.
7. The Medical Times and Gazette. Churchill.
8. The British Medical Journal.
9. The Asylum Journal of Mental Science. Churchill.
10. The Glasgow Medical Journal. Dunn and Wright.
11. The Athenæum.
12. The Dublin Medical Press.
13. The Westminster Review. Trübner.
14. Transactions of Obstetrical Society. London: Longmans.
15. Journal of Cutaneous Medicine and Diseases of the Skin. Edited by H. S. Purdon. Churchill.
16. The Practitioner; a Monthly Journal of Therapeutics. Macmillan and Co.
17. The Journal of Anatomy and Physiology. Macmillan.
18. The Food Journal. London: J. M. Johnson and Sons.

INDIA.

19. The Indian Annals of Medical Science. Calcutta: Barham, Hill, and Co. London: Longmans, Green, Reader, and Dyer.
20. The Madras Monthly Journal of Medical Science. Madras: Gantz, Brothers. London: Hardwicke.
21. Indian Medical Gazette. Calcutta: G. Wyman and Co.

AUSTRALIA.

22. The Australian Medical Journal, Melbourne: Stillwell and Knight, London: H. Baillière.

AMERICA.

23. The American Journal of the Medical Sciences. Edited by Isaac Hays, M.D. Philadelphia: Henry C. Lea. London: Trübner and Co.
24. The American Journal of Science and Arts. Conducted by Professors B. Silliman, and J. D. Dana, &c. New Haven: Editors.

AMERICA.—Continued.

25. The American Journal of Insanity, Utica, N. Y. State Lunatic Asylum.
26. The American Journal of Obstetrics and Diseases of Women and Children, New York: W. A. Townsend and Adams. London: Trübner and Co.
27. The Cincinnati Lancet and Observer. Cincinnati: E. B. Stephens, M.D.
28. Canada Medical Journal. Montreal: Dawson, Brothers.
29. The New York Medical Journal. New York and London: D. Appleton and Co.
30. The Medical and Surgical Reporter. Philadelphia: S. W. Butler, M.D.
31. The Richmond and Louisville Medical Journal. Louisville, Ky.: E. S. Gaillard, M.D.
32. The Medical Record. New York: Wood & Co.
33. The New Orleans Journal of Medicine. New Orleans: W. S. Mitchell, M.D. London: Trübner and Co.
34. The American Practitioner. Louisville, Ky.: John P. Morton and Co. London: C. D. Cazenove.

FRANCE.

35. Gazette Médicale de Paris. Paris
36. Gazette Hebdomadaire de Médecine et de Chirurgie. Paris: Victor Masson.
37. Journal de Chimie Médicale, de Pharmacie, de Toxicologie, et Revue de nouvelles scientifiques, nationales et étrangères, &c. Paris: Labé.
38. Journal de Médecine de L'Ouest. Nantes: Mellinet.
39. Journal de Pharmacie et de Chimie, &c. Paris: Victor Masson.
40. L'Union Médicale. Paris.
41. La Lancette Française, Gazette des Hôpitaux civils et militaires. Paris.
42. Revue Médicale Française et étrangère. Publié par le Docteur Sales-Girons, Paris.
43. Archives Générales de Médecine. Paris: Asselin.
44. Bulletin de l'Académie Impériale de Médecine. Paris: Baillière.
45. Revue de Thérapeutique Médico-Chirurgicale. Paris: Dr. A. Martin-Lauzer.
46. Journal de Médecine et de Chirurgie Pratiques à l'Usage des Médecins. Par Lucas-Championnière. Paris.

List of Exchange Journals.

FRANCE.—Continued.

47. Journal des Connaissances Médicales Pratiques. Paris: J. B. Baillière et Fils.
48. Annales Médico - Psychologiques. Par MM. Baillarger, Cerise, et Lunier. Paris: V. Masson.
49. Bulletin Général de Thérapeutique, Médicale et Chirurgicale. Par le Docteur Félix Bricheteau. Paris.
50. Répertoire de Pharmacie. Par M. le Dr. Bouchardat. Paris: G. Baillière.
51. Gazette Médicale de Strasbourg.
52. Journal de Médecine de Bordeaux.
53. L'Union Médicale de la Gironde, Bordeaux.
54. Annales D'Hygiène Publique et de Médecine Légale. Paris.
55. Lyon Médical Organe Officiel de la Société Impériale de Médecine. Lyon: Mégret.
56. Journal de Médecine Mentale. Par M. Delasiauve. Paris: Masson et Fils.
57. Archives de Médecine Navale. Paris: J. B. Baillière et Fils.
58. Archives de Physiologie Normale et Pathologique. Publiées par MM. Brown-Séguard, Charcot, Vulpian. Paris: Victor Masson, et Fils.

BELGIUM.

59. Bulletin de l'Académie Royale de Médecine de Belgique, Bruxelles.
60. Annales D'Oculistique. Bruxells.
61. Annales et Bulletin de la Société de Médecine de Gand.

GERMANY.

62. Vierteljahrschrift für die praktische Heilkunde, herausgegeben von der medicinischen Facultät in Prag. Prague: Karn André.
63. Canstatt's Jahresbericht über die Fortschritte der gesammten Medicin in allen Ländern. Redigirt Von Pr. Scherer, Pr. Virchow, und Dr. Eisenmann. Würzburg: Stahel.
64. Wochenblatt der Zeitschrift der k. Gesellschaft der Aerzte in Wien (Beilage zu den Jahrbüchern). Redigirt von A. Duchek, C. Langer, A. Schauenstein. Leipzig: Hinrichs.
65. Deutsches Archiv. für Klinische Medicin. Erlangen: Th. Blaesing.
66. Jahrbuch für Kinderheilkunde und Physische Erziehung. Leipzig: B. G. Teubner.

PRUSSIA.

67. Archiv für pathologische Anatomie und Physiologie, &c. Herausgegeben von R. Virchow. Berlin: G. Reimer.
68. Allgemeine Zeitschrift für Psychiatrie und psychisch-gerichtliche Medicin. Herausgegeben von Damerow, Flemming, Roller durch Heinrich Laehr. Berlin: Hirschwald.
69. Berliner Klinische Wochenschrift. Berlin: Hirschwald.
70. Archiv für Klinische Chirurgie Herausgegeben von Dr. B. von Langenbeck. Berlin: Hirschwald.
71. Monatsschrift für Geburstkunde und Frauenkrankheiten. Berlin: Hirschwald.

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PART I.

ORIGINAL COMMUNICATIONS.

ART. IV.—*Dr. Sayers' Splint applied to Fractured Neck of Femur.*

By WILLIAM COLLES, F.R.C.S.I., Surgeon to Steevens' Hospital, and Lecturer on Surgery in the Hospital College.

THE various applications recommended at different times, and the continued alterations proposed in the form and adjustment of splints for the treatment of fracture of the femur sufficiently indicate that a proper apparatus for these injuries is still a desideratum.

This want is more apparent in the treatment of fracture of the neck of the femur than most others: here we have to deal with, in general, an infirm old female whose health is failing, who is subject to various bronchial affections, whose bones have become more liable to fracture, whose flesh is more apt to suffer from pressure or damp—all which are not found in younger persons; but besides all this, the surgeon cannot insist on the necessity of strict obedience to his directions as to the use of position and splints, as they rarely afford a prospect of obtaining a sound and useful limb.

Still we do not like to abandon these cases, and to leave them without any support or extension, or some effort to assist nature's efforts at union—bony or ligamentous.

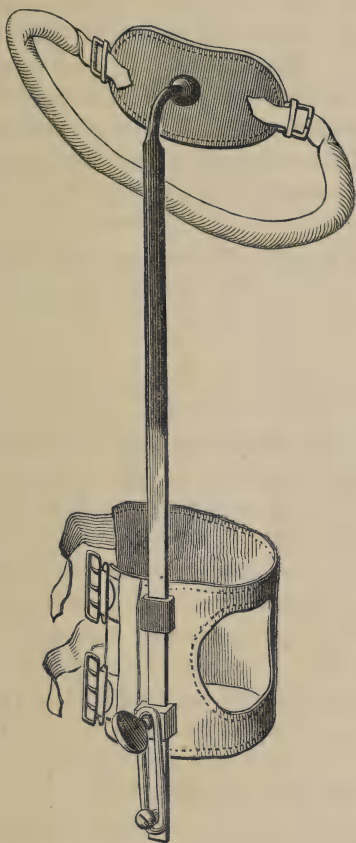
We will not now allude to many of the various positions and appliances recommended for the treatment of this affection, as I believe the majority of surgeons use the long splint, Liston's, or some of its various modifications; this, in a fractured neck of femur

in old persons, is liable to the objections—first, that it keeps the patient's body and limbs in a perfectly straight line, preventing any motion of the patient, so that we are often obliged to leave it off or relax it, to change the position of the patient for purposes of cleanliness or comfort; or, as often happens, the breathing becomes affected, and the patient requires to be placed in a sitting or semi-erect position, and we must again relax the splint. The perineal band is also a source of suffering, causing inflammation, ulceration, or even sloughing beneath it; for it has to bear not only the pressure of the extension, but much force applied to it is lost; for if we consider the action of the long splint fixed to the body by the perineal bands, and also a girth round the pelvis, when these are tightened they act on the trochanter as a kind of fulcrum, and much force is expended in pressing this projection of bone inwards towards the pelvis, and this pressure on the fracture under consideration must add to the patient's suffering, pressing the two rough surfaces of bone together, and may interfere with the process set up for union either by ligament or bone.

I have long tried various forms and kinds of splints for the treatment of this injury without meeting any one to give satisfaction, till I read the interesting lecture of Dr. Sayers, recorded in the *British Medical Journal*, where he describes his splint for morbus coxæ, where the extension is fully kept up, and the patient is enabled to bear the weight of his body on the instrument with ease and comfort.

Having been called to a case of fractured neck of the femur in an old lady, I resolved to try a modification of this splint, so as to make it suitable to the injury, and quickly and cheaply constructed. The instrument consisting of three parts—first, a pelvic portion about two inches deep and six inches broad, padded and falling to the brim of the pelvis lying over the trochanter and pressing on the os innominatum; this must be retained in its place by a perineal band. 2nd. The thigh splint—a bar of iron half an inch broad, joined to the pad by a ball and socket-point—stands out horizontally for an inch or so to carry it beyond the trochanter, where it bends down and lies close to the femur, and projecting an inch or two beyond this bone. The third portion is the knee-piece—a broad band of iron embracing the front and inner portion of the knee, the remaining circle of the limb being held by a strap and buckle. This knee-piece slides along the thigh splint, and may be fixed by a screw, or, what I prefer, a strong elastic band passing

from the screw head to a knob fixed on the lower end of the splint, because we have a constant and steady extension kept up which we can increase or lessen as we find necessary. The instrument was made for me by Messrs. Adam and Corcoran, Ormond-quay. This splint is certainly open to the objection of the perineal band, but the pressure on this is reduced to the minimum, because the extending force acts on the pelvic pad, and the stronger it is the more it presses this pad against the pelvis, so that the entire strain is not on the perineal band as in the ordinary splint. In the case in which I tried it the splint gave me great satisfaction:



it kept the limb well *in situ*, prevented the common shortening and eversion, and was not inconvenient to the patient; and I have no doubt but that it will be found useful not only in this fracture

but I should think in many other fractures of the bone, and might even be utilized in artificial limbs, where we wish the pressure to be borne on as extensive a surface as possible—for which purpose I recommend it to the notice of the profession.

ART. V.—*New Method of Destroying Faulty Cilia in Cases of Limited Trichiasis or Districhiasis.* By PATRICK J. HAYES, Surgeon to the Mater Misericordiæ Hospital, &c., &c.

DURING the early portion of the present session I had under observation, in the Mater Misericordiæ Hospital, two patients suffering from partial districhiasis. In each case the affection was of long duration, and operative measures had been employed by surgeons of experience and skill with a view to remove causes, not only of great distress, but which had produced in both instances a certain degree of permanent corneal opacity, with impaired visual power.

I do not consider it essential to enter into a very detailed account of either case, save in so far as the special treatment which I adopted is concerned.

CASE I.—This patient was a young countryman, of strong, though rather strumous aspect. He stated that he was really of hardy constitution, and greatly enjoyed such exercise as attending gentlemen when out shooting; in fact, he was placed under my care by a retired military surgeon, who formed his acquaintance during the partridge season. The history given was, that for several years his eyelids and eyes had been weak and liable to frequent attacks of inflammation. This ultimately produced very marked trichiasis in the upper lid on the right side, and limited districhiasis of the left. The condition of the right eyelid gave rise to corneal ulceration, and at length the patient applied for relief to an able and well-known surgeon in the South. This gentleman at once removed the margin and hair follicles of the right lid, with lasting benefit to the sufferer, who, however, would not submit the left palpebra to similar treatment, especially as the faulty cilia were comparatively few, and he had learned to remove the most troublesome with a cilium forceps. On examination I found he had opacities of the right cornea, resulting from old ulcerations; also a slight amount of symblepharon and blepharo-phymosis; but

the latter conditions did not produce any real inconvenience. The left eye was somewhat irritated in consequence of the backward direction of cilia growing near the outer part of the upper eyelid, for though the patient had pulled out several large hairs, yet some fine and short ones often escaped the grasp of his forceps.

I informed the man that by a slight operation I believed the irritating eyelashes might be removed, but that nothing could be done to improve the condition of the right eye. This opinion afforded him some consolation, and he at once consented to the treatment proposed. I desired that in the meantime he should not remove any of the growing hairs, as they would guide me in adopting measures for their extirpation. Deeming the case a suitable one for the operation proposed by Herzenstein, I passed a subcutaneous ligature from the palpebral margin, so as to enclose the roots of the offending cilia; but the result was, to a certain extent, incomplete, partly because, in spite of my directions, some lashes had been extracted, and, therefore, the position of their follicles could not be ascertained when the ligature was being passed, but also, an irregular line of very small hairs existed so close to the posterior, or conjunctival edge of the free margin of the lid as to render their obliteration by Herzenstein's method quite impossible. I now determined to employ subcutaneous injection of tincture of the perchloride of iron, which I knew from experience might be so used as to destroy the vitality of a very limited amount of tissue. I performed the operation in the following manner:—A Desmarres' entropion forceps was applied to the eyelid, and so compressed as to cause arrest of circulation through the included portion. Next, a fine hypodermic syringe, charged with a few drops of the tincture of perchloride of iron, was passed obliquely through the palpebral margin, pushed to a point just above the roots of the abnormal cilia, and the tincture slowly injected, as it were, against the follicles. This proceeding was repeated in such a way that the two oblique punctures included the group of cilia to be destroyed. After a few minutes the forceps was loosened, the lashes carefully pulled out, and the eye bathed with cold water. The patient suffered no inconvenience from the operation until the third day, when inflammatory swelling of the lid appeared. This was treated with bread and water poultices, until ulceration of the palpebral margin permitted the escape of a narrow slough of subcutaneous tissue, with complete removal of the hair follicles. The patient soon left hospital, expressing great

satisfaction, and stating he never had expected to feel his eye so strong and free from all irritation.

CASE II.—My second patient was a young woman who had been previously under the care of a distinguished surgeon in one of the principal hospitals in this city. According to her statement she suffered for a considerable time from trichiasis affecting the lids of both eyes, which produced chronic conjunctival inflammation and superficial corneal opacities. About eighteen months before her admission to the Mater Misericordiæ Hospital she consulted the surgeon already alluded to, who most skilfully performed Arlts's operation; but although the patient consequently derived great ease and benefit, yet a few cilia, forming a limited districhiasis, continued to cause considerable annoyance to the right eye. As the other patient was at the time in hospital, and afforded most satisfactory evidence in favour of the injection treatment, I adopted a similar method in this case, and with like permanent relief.

The chief points demanding attention for the safe injection of tincture of the perchloride of iron are:—Firstly, That the eyelid should be firmly compressed between the blades of a suitable entropion forceps. Secondly, That the fine tubular needle of the hypodermic syringe should be passed, and applied, as closely as possible, to the deep ends of the hair follicles. And lastly, That only a very small quantity of the tincture should be introduced.

If those rules be adhered to the operation affords a simple, comparatively painless, and effective means for producing the required extent of madarosis.

ART. VI.—*The Bearing of Anomalous Anatomy on the Evolution Theory of the Origin of Man.* By ALEXANDER MACALISTER, M.B., Professor of Zoology, T.C.D.

THREE arguments deduced from anatomy are advanced in support of the evolution hypothesis of the origin of man; these may be called—1st, the argument from embryology; 2nd, the argument from rudimental organs; and 3rd, the argument from anomaly; of these the second and third merge into each other; and I desire in the present paper to examine the last of these arguments with a view to ascertain its true force.

This argument may be stated thus:—It is the experience

of anatomists that in man's body, organs are variable in position, relative degrees of development or arrangement; and as these varieties in man are supposed often to simulate the ordinary disposition of parts in the lower animals, it is inferred that this indicates the existence of a genetic affinity between man and the other mammals. This argument is used by Mr. Darwin in his book on the Descent of Man.

Of all parts of the human body the muscular system is that which the most frequently exhibits varieties; thus I have collected notes of over two thousand forms of anomaly which have been hitherto described in human myology; and as the normal muscular arrangements in lower animals are usually of easy recognition and are pretty well known, the first point is to determine, do these abnormalities point towards characters distinctive of lower animals?

The proper method for the ordering of our facts in attempting the determination of this point is to classify in relation to it muscular anomalies, to see whether they are all explicable on the hypothesis of a law of Reversion, and if not, to ascertain what are the other factors by whose agency the existence of anomalies is brought about. The first class of varieties considered in this respect consists of such muscles as are not normal elements of the average human body, such as the occipito-scapular, sternalis brutorum, levator claviculæ, epitrochleo-anconeus, peroneus quinti, abductor ossis metatarsii minimi digiti, dorsi-epitrochlear, &c. Now, of these the first exists in the majority of the primates, except man and the chimpanzee; the second is not so universal, but does occur; the third exists in all but man; the fourth, though present in a few of the lower primates, yet is not, as a rule, present in man or in the nearest of his allies, the catarrhinæ; the fifth peroneus is present in all but man as a rule, and its rudiment is of very frequent occurrence as an anomaly in the human foot; the last of the series is constant in all but man: in him, cases of its occurrence have been noticed by Bergmann, Henle, Theile, Halbertsma, Wood, and myself. Thus, of the seven examples of this class of anomalies, taken by way of illustration, three occur normally in all the primates but man, one occurs in all but man and the chimpanzee, and the remaining three occur only in some of the primates, and most frequently in the lowest or least anthropoid species. All these varieties, then, would be explicable on the hypothesis of development, by the law of reversion.

Are there any muscles of this class not admitting of such an

explanation? Certainly there are; take, for example, the large group of laryngeal occasional muscles. Of the entire group of sixteen—[thyrohyoideus minor or azygos (Sömmerring, Morgagni, Weber-Hildebrandt, Haller, Gruber); kerato-cricoid, crico-hyoid (Zagorsky); kerato-hyoideus (Gruber); thyreo-syndesmicus (Sömmerring); thyreo-triticeus (M.), hyo-trachealis (Gruber: Reichert's *Archiv.*, 1868, p. 644); thyro-trachealis (*ibid.*, *Bulletin de l'Acad. Imp. de Petersb.*, tom. 3, p. 153, 1861); crico-trachealis (M.); thyroideus transversus anomalus (Gruber: *Neue Anom. Kehlkopfs-muskel. Oesterreich Med. Jahrbuch*, Bd., 52, 1845, p. 148); kerato-arytenoideus (*ibid.*, Reichert's *Archiv.*, 1868, p. 640); hyo-epiglotticus, thyreo-epiglotticus major and minor (*Tarin Myographie*, p. 62); crico-epiglotticus (*Verheyen*, p. 188); glosso-epiglotticus (Eustachius)]—only five have been found in animals: the thyroideus transversus anomalus, in *Hylobates albifrons* by Eschricht (Müller's *Archiv.*, 1834, p. 218); the kerato-arytenoideus I found in a chimpanzee, the hyo-epiglotticus, thyreo-epiglotticus, and glosso-epiglotticus also occur in lower animals. Of course it is possible that homologues of these may be found in some animals; but, as far as I know, none have as yet been met with, so that a reversionary theory would fail to account for their presence.

2. The second class of varieties as considered in the light of evolution consists of failures of the specific human-muscle-forms. Cases have been described of absence of the peculiarly anthropoid muscle arrangements, which are—1. The coronoid head of the pronator teres; 2. The radial head of the flexor digitorum sublimis; 3. The tibial origin of the solæus; 4. The peroneus tertius; 5. The presence of a perfectly separate flexor pollicis longus; 6. And of an extensor primi internodii pollicis. Of these the first and third have been found in the chimpanzee by Professor Humphry, as anomalies in the myology of that animal, for Vrolik, Wilder, Moore, and I, find no trace of such heads in the specimens dissected by us, and Professor Humphry also failed to find them in another specimen. Such instances of germ suppression or failure of peculiarly human characters are of course, like all other arrests of development, easily explained on an evolution hypothesis.

3. The third group of varieties is that which includes all the examples of structures normally human, arranged according to plans observed in lower animals, such as—1. The varieties of the digastric muscle, the anterior bellies joined by a transverse tendon or decussating in the middle line, which forms are normal among

the primates. Another form of this muscle, as a monogastric depressor of the mandible, is found in still lower forms of the mammalia; as a human anomaly, it was described by Platner. 2. The varieties of the lesser pectoral; these have also their representatives among the primates as normal arrangements; thus, De Souza's variety, or that in which it is inserted into the greater humeral tuberosity, is the common mode of its arrangement in the catarrhine monkeys. In one chimpanzee Professor Humphry found it inserted into the humerus, crossing the coracoid process; in Professor Wilder's animal it was attached to the humerus on the right and into the coracoid on the left. In one chimpanzee I found the muscle attached to the coracoid process on both sides; and in a second I found it attached to the coraco-acromial ligament on the right and to the coracoid on the left. 3rd. The insertion of the extensor minimi digiti into the fourth and fifth fingers in man is not uncommon, and this is the commonest mode of insertion in the quadrumana. In the chimpanzee, however, as in man, it is restricted alone to the little finger.

4. The existence of a middle finger slip of the indicator. This is also a common anomaly, and is a universal character among the higher primates; it was only known to fail once in the chimpanzee by Wilder.

5. Some varieties in position of the flexor tendons of the hand coincide with the arrangements among the catarrhinæ; thus the form in which the radial deep flexor is indicio-pollicéal in its insertion, resembles the arrangement found by Wilder in his chimpanzee. Examples of this class of varieties might be multiplied, but I confine myself to these as illustrations.

Are there any anomalies of this class which cannot be accounted for on the theory of reversion? Perhaps a few may be in this category, such as the not uncommon and very well known pterygoideus proprius, a case of which was recorded by Mr. Wagstaffe in the May number of the *Journal of Anatomy and Physiology*, 1871.

In analysing these and many other forms of anomaly, in order to determine to what factors the occurrence of muscle-anomalies is due, there are, to my mind, only two hypotheses which offer any intelligible explanation of their mode of production; the first of these is based on considerations of function, the second is purely morphological.

With regard to the first of these, if it were the sole explanation of the occurrence of anomalies, then we would find such muscles as

present the greatest complexity of function to be not only the most variable but the only muscles exhibiting a great range of variation. It is true that such muscles are very variable, as exemplified in the case of the flexor sublimis digitorum, digastric, omohyoid, lumbricales, &c. But no such consideration could account for the enormous variability of the biceps or of the palmaris longus; the former of these has certainly not more than four simple functions—supination, flexion of the forearm, extension of the shoulder, and tension of the antibrachial aponeurosis; yet its varieties enormously exceed those of the triceps, which has a double function, in the proportion of thirty to five in number of forms of variation, and thirteen to one in absolute frequency of variation; while the brachialis anticus, with its single function, is twice as variable as the triceps.

That functional variety is a factor and an important one in the production of anomalies is probable, however, from three considerations:—

1st. Structures like the laryngeal muscles, which are enormously varied in their degrees and combinations of actions, are exceedingly variable, as the list of anomalous laryngeal muscles (*supra*) will show.

2nd. Such rudimental parts as have their functions obsolete are exceedingly variable. A case in point to illustrate this is the example of the forearm muscles in the whale *Balænoptera rostrata*. Two specimens of this animal have been dissected with care; in the first I drew and described carefully the forearm muscles; in the second Mr. Perrin, with equal care, examined and described them, and in many respects the two were different. I have been informed by Professor Struthers of Aberdeen that he found a still different arrangement in *Bal. musculus*, and by Dr. Murie that he examined the corresponding part in *Physalis antiquorum* and found none at all, as I had also found in *Globiocephalus Svineval* and in *Delphinus*.

3rd. Muscles with definite single functions, though not invariable, yet are the most stable in form, and present the fewest anomalies, *e.g.*, the rectus femoris, vasti, cruræus.

But this functional consideration (that anomalies are due to the hereditary transmission of forms produced by varieties of action or use), we can only regard as one factor in the production of anomaly, as it is utterly inadequate to account for the formation of many varieties, which, instead of being advantageous for the generality of

actions, are positively injurious; thus the indicio-polliceal deep flexor, muscle absences, a cubito-radial insertion of the biceps, the *achselbogen*, the insertion of the digastric into the angle of the jaw, are undoubtedly prejudicial to many of the functions of their respective parts, and scarcely, if at all, beneficial to any, while they are all well-known lower animal forms. If the second factor be not a tendency to reversion, what is it?

Two explanations are sometimes given of such anomalies. One of these is, "that they are accounted for on the hypothesis of a fundamental unity of plan in animals." But if we analyse this as an explanation, we will find that though the expression enunciates an undoubted fact, that such a unity of plan exists, yet it contains no information or interpretation as to the cause of the unity, which, after all, is the point of which we are in search; for if muscle anomalies be (as they are) the accidental production in one animal of the normal structures of another, it is the cause of this, whatever it may be, final or secondary, which must determine the production of anomalies.

A second explanation is that such anomalies are accidental arrests of development, retentions of fundamental structures, but that this is wholly inadequate can be easily understood if we consider that the largest proportion of anomalies are never normal embryonic conditions in man—certainly none of the supernumerary muscles are so.

We are thus placed in this position: we must account for anomalies on one or other of the following grounds:—

1. Variety depending on variation of function.
2. Depending on evolution or on the plasticity of form and liability of the frame to return, in some of its details, to more primitive arrangements. In this sense alone are anomalies explicable as arrests of development.

Or else we must confess ourselves thoroughly incapable of explaining their occurrence.

A high authority, Dr. Carpenter, has warned us of the danger of hasty theorizing, but I think we should not condemn all theory. Under proper regulation a hypothesis is a most valuable aid in our studying and grouping of facts; besides, our consciousness is so conditioned that we cannot conceive of anything produced, that is, an effect, without its necessary and logical condition of a cause; these anomalies are remarkable phenomena, and the more closely they are studied the more it will be perceived that they are capable

of definite classification, and that their production is apparently under definite laws, and a detailed study of individual abnormalities will, I believe, be found to bear out strongly the general principles herein enunciated.

ART. VII.—*Five Hundred Cases of Re-Vaccination.* By RICHARD J. HALTON, L.K. & Q.C.P.I., L.R.C.P.E., L.R.C.S.I., Physician to the Dispensary, and Medical Officer of Health to Kells.

WHEN the alarm, caused in Ireland last spring by the fearful mortality of the small-pox epidemic in England, was at its height, and it was feared by many amongst us with what has unfortunately proved a well-grounded fear, that we would not escape a visit from the disease, the majority of the cases with which this paper deals presented themselves at the dispensary for re-vaccination.

At that time re-vaccination was held to be a tolerably efficient protection against the small-pox.

Having heard that this opinion has now been abandoned in several quarters where the highest professional eminence has given great authority to that abandonment, I deem it the duty of every one who has had any experience of the question to give the results of that experience.

This I will endeavour to do for my part, as shortly and succinctly as possible, premising that my experience has not been confined to the number of cases heading this paper, but I take them as they were for the most part observed with scrupulous care, and the results, where possible, accurately noted.

As each case presented itself, the state of the arm was noted in reference to vaccination marks, and the result was that 179 had scars that were mostly circular, and did not seem to go deeper than the true skin. 106 had scars so slight that they had often to be carefully looked for, amounting in many cases to a mere puckering, and 66 had deep scars that were mostly irregular in shape, and seemed to penetrate through the skin into the cellular tissue beneath.

In 24 of the cases no scar was to be found, though they stated themselves to have been vaccinated in infancy.

Out of the whole number 50 stated that they had never been vaccinated, and of these 8 were born since 1864.^a

I have excluded 75 cases from the above enumeration, as the state of the arm was not recorded with sufficient accuracy.

The cases were all vaccinated in the usual way, from healthy infants with well-formed vaccine vesicles, and they were requested to present

^a When the Registration first made compulsory vaccination possible.

themselves for examination on that day week. The great majority complied, but 143 of the cases presented themselves at irregular times, that is on other days than the eighth, and some few of the number failed to present themselves at all, and have been consequently excluded.

The results may now be briefly detailed. Of 179 who had, when re-vaccinated, circular scars going to the depth of the true skin, 10 presented perfect vaccine vesicles, pearly in colour, inclining to the circular in shape, depressed in centre, filled with clear lymph, and without, or with very little areola. Their ages varied from 7 to 15.

All the others of that class presented the imperfect vesicle in its various forms, sometimes white like a blister, sometimes filled with yellow serum, sometimes a pustule, once a tubercle, and often a scab. There was usually considerable inflammatory redness, stretching down the arm, sometimes to the elbow; but beyond a little itching and an occasional feeling of tightness, no distress was experienced.

Of the 106 who presented slight puckered marks, 9 had, on the eighth day after vaccination, perfect vaccine vesicles. The remainder presented irregular vesicles as above.

Of the 66 cases who had deep scars, 6 showed the perfect vaccine vesicle. Their ages ranged from 7 to 15. The remainder showed imperfect vesicles.

Of the 24 cases who stated themselves to have been vaccinated in childhood, but who had no scars, 16 presented the perfect vaccine vesicle; 2 the imperfect vesicle, and the remaining 6 failed to present themselves on the eighth day. There were some in each class who so failed, but whom I saw at other times.

Eighteen of all the cases failed to take on the first occasion of being vaccinated, or at least did not show any evidence of action on the eighth day. On the operation being repeated, it was, in nearly all the cases, successful. The vaccine matter was introduced by making several parallel scratches with a blunt lancet, so that the blood appeared at the bottom of the scratch, but did not flow. The lymph in the greater number of the cases was then taken carefully as it flowed out from the *summit* of the vesicle on the arm of the child who supplied the infection, and rubbed into the scratch with the flat of the lancet. Great care was taken that no blood was mixed with the lymph.

In some cases it happened, in spite of the greatest care, that, in opening the vesicle, a drop or two of blood escaped along with the lymph. By leaving the vesicle untouched, however, for a minute or two, the blood coagulated, and then the lymph could be taken, without any admixture, flowing out clear and pure beside it. The operation caused no pain, and in the whole number of cases, and in many more that do not come within the scope of this paper, there did not occur a single instance of its being followed by any unpleasant complication, as swelled glands, abscess, &c.

The arms now and then *looked* very sore and angry; but the patients assured me that, beyond the tightness and itching, mentioned above, they suffered nothing.

The constitutional disturbance, if any, was so slight that it passed without notice.

I may here mention one incident, though it does not belong to re-vaccination, which is of value in showing the care that is required in performing this little operation.

Most of the cases here recorded being done on two or three several days, there was consequently a great pressure of work at the dispensary, and I was obliged to procure assistance in vaccinating them. On the first day I observed one of my assistants, in vaccinating a baby, pressing the flat part of the lancet on a vesicle, already exhausted by previous vaccinations, to try and squeeze some matter from its base. I remonstrated with him, but not soon enough to prevent the mischief, as he had the operation just completed. I, however, took particular note of the child, a fine healthy infant, just six months old. It was brought to me some days after, covered with a moist eruption, with brown scabbing, and suffering from a good deal of constitutional disturbance. Not one of the other children who were vaccinated along with it from the very same infant, while the lymph was freely exuding, showed the slightest sign of eruption. When I have heard since of cases where re-vaccination produced considerable inconvenience, swelled glands, &c., &c., it struck me forcibly that it might be owing to some similar carelessness on the part of the operator.

Having now detailed our precautionary measures, I will proceed to state the results, up to this time, on the public health of the town. Situated as we are in Kells, in constant railway communication with Dublin and Belfast, we could scarcely hope to escape altogether while the small-pox was so prevalent in both places.

In effect, we have had two cases of small-pox imported, one from either city, who were at once removed to hospital, on their arrival here; but, except in one instance, the disease was not communicated. In that one case the subject was a boy, about twelve years of age, who lived opposite the hospital where the small-pox patient was conveyed.

He was the only member of his family except the baby (three months old) who was unprotected by vaccination; but by some neglect it was omitted in his infancy; and though his father repeatedly urged him to present himself at the dispensary, when the re-vaccinations were in progress, a boyish fear of the supposed pain of the operation prevented him. He paid dearly for his folly. He had been out playing in the neighbourhood of the hospital one day, and he came home in the evening sick, with a severe pain in his back. It was supposed, at first, that he had hurt himself leaping, and consequently I heard nothing of

the case for two days. I saw him on the evening of the third day, and had him at once removed to hospital. The next morning the eruption of small-pox appeared.

I at once re-vaccinated all his family, and vaccinated the baby. I also vaccinated and re-vaccinated the people in the neighbouring cabins, and took the sanitary measures that appeared necessary; had the houses lime-washed inside and out; ground and sewers flushed with carbolic acid solution, &c., &c. Not another case appeared; and, as three months have now elapsed, we may be considered to have escaped that centre of infection. The poor boy himself has recovered from a bad attack of confluent small-pox, deeply pitted and scarred, and with the loss of an eye.

When it is considered that the whole family live in a wretched single-roomed cabin, and slept altogether in one bed with him for those two nights, father, mother, and four other children, their escape will appear very remarkable.

There is one curious circumstance about the baby, however, which is not without interest. It will be remembered that it was nearly three days in almost actual contact with the sick boy before it was vaccinated.

The vaccine ran the usual course, showing a perfect vesicle; but, a fortnight after, the mother brought the child to me to show me two pustules, exactly resembling the pustules of small-pox, one situated on the upper lip and the other at the junction of the *alæ nasi* with the cheek on the right side. They have since healed, but have left two marks behind them.

I may shortly mention another case which, if less striking, is not devoid of interest.

A lady, who lived some miles from this, had a near relative in Dublin ill with the small-pox. She went up to bring the invalid home to breathe the pure air of the country during convalescence, and, as a precautionary measure, all the family and servants were re-vaccinated. But this poor lady, from some prejudice she had conceived against the operation, withstood the most urgent entreaties of her family and medical attendant (from whom I have the case) to allow herself to be thus protected.

Two days after her return she sickened, and in five days was dead from a very malignant form of small-pox.

Not a single member of the family, and not one of the servants, though inhabiting the house, attending the sick room, &c., &c., took the disease.

Before I conclude, I may mention that I always insisted on perfect repose of the re-vaccinated arm, as far as that was possible, and enjoined, in the case of adults, abstinence from stimulants.

I may also say that I could not in any case succeed in producing any

approach to a perfect vesicle in a vaccinated child under seven years old, indeed in those cases the lymph seemed to excite very little action of any sort.^a

I think that from the foregoing facts the following conclusions may be justly drawn:—

First,—That a large proportion of the population are utterly unprotected by vaccination.

Second,—That vaccination scars, no matter how deep, are no guarantee that the possessor is protected by vaccination.

Third,—That in some constitutions the vaccine influence wears out, as is clearly shown on re-vaccination, when a perfect vesicle will be produced.

Fourth,—That the operation of vaccination, when properly performed with lymph, taken from the *summit* of a vaccine vesicle, without areola, on the eighth day, from a healthy child is entirely unattended with danger, and in the majority of cases with very slight inconvenience.

Fifth,—That there are reasonable grounds for believing that re-vaccination is, to a considerable extent at least, an efficient protection against small-pox.

ART. VIII.—*Observations Illustrative of the Use of Dieulefoy's Pneumatic Aspirator.* By DR. LAFFAN, Physician to the Union Hospital, Cashel.

THE following case is offered as a contribution towards determining how far the above instrument supplies a long felt want in the diagnosis and treatment of deep-seated collections of fluid. The means hitherto at our disposal for their diagnosis and treatment have been so little exact, or attended with so much risk, as to require our most careful attention for any invention which promises to make them more perfect. Previous to using M. Dieulefoy's instrument on the living subject, I carefully investigated its powers of *aspiring* fluids of different densities outside the body. The results were such as to convince me of the capacity of its finest trochars to aspire fluids of a density equal to that of ordinary abscess contents.

J. B., aged 35, was admitted into the Cashel Hospital 25th of March, 1871. His complexion was dark, his appearance spent, and he was just convalescing from fever. His pulse was 92, respiration 28. There was fulness and diffused tenderness in the hepatic region, with some bronchial râles confined to the right side, and rusty purulent sputa.

^a When performing vaccination, I always wear a large soft handkerchief at the button hole, as when using the laryngoscope. In this, after each operation, I carefully wipe the lancet, after having dipped it in water, as recommended by Dr. Labbatt.

He felt some difficulty in lying on the right side. His sleep and appetite were fair. His bowels were generally confined. The other organs were healthy. His previous history was as follows:—

He had been a soldier, and had served in India. There he led an intemperate life; had contracted syphilis, for which he had been mercurialized, and finally had been seized with symptoms of inflammation of the liver. These symptoms, despite of the most active treatment, grew worse, and he was finally sent home to Netley, as a case of hepatic abscess. Here the abscess, according to his own statement, pointed, and it was proposed to open it. He declined, and left the hospital. The swelling, instead of bursting, declined away by degrees; his general symptoms abated, and he was able to work as a groom for some months. He then got a wetting, and contracted what appears to have been typhus fever. He recovered from this, and was admitted under my care as above described. The absence of any positive evidence of abscess, and the presence of a lung lesion in itself sufficient to account for all the symptoms, threw the chief weight of diagnosis on the patient's own statements. He was ill enough—abscess or no abscess—to require admission into hospital. There was, therefore, wanting the usual motive for misstatement. I inclined to the view of abscess, and he was put under the course of treatment which seemed most suited for such a case. The symptoms continued for some months without change. On the 3rd of May he lost a considerable quantity of blood per anum. Some also came by the mouth, but it was not clearly made out whether it was derived from the stomach or lungs. On the 26th of June he was seized with such violent and profuse vomiting, and such profound collapse, as to lead to a momentary belief that the abscess had at length burst into some of the great cavities. This vomiting continued with diminishing violence for some days, when it ceased. Some time after this, several cases of thoracentesis published in the French journals, having drawn my attention to the “aspirateur,” I determined to give it a trial. The instrument, we were told, was safe even if no abscess existed here; its use, therefore, could do no mischief. If an abscess did exist, and the balance of evidence favoured this view, its presence was the source of hourly and deadly peril to the patient. It appeared to be an imperative duty to try a remedy which promised to be a safe one, in a case in which the do-nothing system (for any treatment of the case not embracing surgical interference I hold to deserve such a title) was likely to end at any moment in the destruction of the patient.

Accordingly, on the 19th of September, chloroform having been previously administered by my dresser, Mr. W. Hourigan, the No. 1 (the finest trochar of the aspirateur) was inserted up to its full length between the seventh and eighth right ribs in a line with the angle of the scapula. Some blood only entered the receiver. The patient felt a little tenderness

in that side, and during that night did not lie on it; but neither the pulse, temperature, nor any external appearance gave any indication of the slightest mischief to the system. After a day even the slight symptoms enumerated passed away, and he was just as before. As the negative results obtained might have arisen from the small size of the abscess, I determined to try such a number of tapplings as would leave no part of the organ unexplored. On the 30th of September, the part having been this time frozen by means of Richardson's apparatus, the No. 1 trochar was inserted in two places, first, between the sixth and seventh ribs in a line with the nipple, and secondly, between the same ribs in a line with the axilla. On this occasion no blood even entered the receiver. Some little tenderness and indisposition to lie on the side followed, but no evidence whatever of any constitutional mischief was present. After a day the patient was in every respect as before.

On the 10th of October the No. 1 trochar was for the third time inserted—first, between the ninth and tenth ribs in a line with the spinal border of scapula; second, between the tenth and eleventh ribs one and a half inch nearer the spinal column; and third, in the epigastrium below the ensiform cartilage. No fluid of any kind found its way into the receiver. The patient was as entirely unaffected by these operations as on the first two occasions. The number and arrangement of these tapplings were such as to ensure the thorough searching out of every part of the organ to which suspicious symptoms had at any time been referred. Judging by the results obtained outside the body, as well as from other trials of the No. 1 trochar, I felt satisfied that there were no abscess contents to be drawn off; and that either the abscess had been absorbed, or that the original diagnosis was inaccurate. I have with the same delicate trochar evacuated the contents of dropsical knee-joints with the most perfect success. I have, however, on other occasions not been so successful; why I know not. The fluids were of the same density, and the circumstances apparently identical. My experience, which now embraces several trials of the Nos. 1, 2, 3, and 4 trochars of the aspirateur, leads me to conclude that while No. 1 is not always certain, with Nos. 2 and 3, and with care, serous fluid can be always—purulent fluid nearly always—drawn off with the most perfect immunity from all ill consequences to the patient. Had fluid been present in the liver in question—and the progress of the case since has shown that it was not present—its evacuation could have introduced no new element of danger to alter the result. There would have been, certainly, not more irritation, for the organ would have been no less tolerant of interference. There would have been no danger from the entrance of air, as, against that, the instrument is certainly a complete protection. Escape of the abscess contents into the abdominal cavity—an occurrence which has proved one of the chief elements of mischief in ordinary tapping—would

have been prevented by the capillary nature of the opening. It could not have taken place while the opening was filled by the trochar, nor could it have happened at a subsequent period, as the muscular parieties of the opening would, as in the case of punctured intestine, have closed it up before the fluid would have collected again. The one positive result which this case goes to establish is the perfect safety with which the most important organs may be traversed, not in one, but in several directions, by these delicate instruments. The *aspirateur pneumatique* is indeed susceptible of many improvements, but the length of this article precludes me from entering upon them. I shall have achieved all that I have proposed for this notice of the aspirateur, if I shall have induced some who have not hitherto availed themselves of its advantages, to use it and to improve it.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

RECENT WORKS ON SYPHILIS.

1. *Practical Lessons in the Nature and Treatment of the Contagious Diseases ; an Account of the Primary Syphilitic Poison and of its Communicability. With an Appendix on the recent Report of the Royal Commission on the Contagious Diseases Act and its Application to the Voluntary Hospital System.* By JOHN MORGAN, M.D., F.R.C.S.; Surgeon to Mercer's Hospital and to the Westmoreland Lock Hospital, Dublin, etc., etc. London: Baillière, Tindall, & Cox. 8vo. Pp. 335. 1872.
2. *Notes on Syphilis, with an Appendix on the Unity of the Syphilitic Poison.* By S. MESSENGER BRADLEY, F.R.C.S.; Lecturer on Comparative Anatomy, Royal School of Medicine and Surgery, Manchester. London: Churchill. 8vo. Pp. 48. 1872.
3. *On Syphilis.* By CHARLES R. DRYSDALE, M.D., M.R.C.P., Lond., F.R.C.S., Eng.; Physician to the North London Hospital for Consumption and Diseases of the Chest, the Metropolitan Free Hospital, &c. *Transactions of St. Andrew's Medical Graduates Association.* 1870.
4. *Syphilis; its Nature and Diffusion popularly considered.* By JAMES GEORGE BEANEY, F.R.C.S.; Late Honorary and Consulting Surgeon to the Melbourne Hospital. Melbourne: George Robertson. 8vo. Pp. 303.

SOME time ago in reviewing the works of Lancereaux, Barton, and Berkeley Hill, we discussed at considerable length various questions connected with venereal disease, more recently we had before us Dr. M'Donnell's Lectures, and we now have the works which stand at the head of the present article.

The position and ample opportunities of studying venereal

diseases enjoyed by Dr. Morgan entitle his treatise to very great attention, and we regret that the unusual pressure on our space prevents us giving it the full notice we would desire.

The authors whose views were last before us were unanimous in the opinion that there were two perfectly distinct venereal poisons capable of producing sores on the genital organs, one which caused within a day or two of its application a sore the local consequences of which might be very distressing, but the effects of which ended there, and another which produced, after an incubation of about fourteen days, a sore which was in its turn followed by eruptions on the skin and various other manifestations of profound contamination of the system. They affirmed that these sores were so different in appearance that in the vast majority of cases the surgeon could distinguish the one from the other, and although occasionally this might not be the case, just as the physician now and then sees a case which presents some of the signs of scarlatina and some of the signs of measles, they claimed to have established the doctrine that these two venereal diseases were essentially distinct maladies; such has always been our opinion, for we believe the evidence of a fundamental difference between the two kinds of venereal sore is as strong as that which compels us to separate typhus from enteric fever, and scarlatina from diphtheria; the grounds of this opinion and the distinguishing marks of the two kinds of sore we set forth in the review to which we have already alluded; the reverse doctrine however, the doctrine of the essential unity of the chancrous virus, has not been without its supporters, and by none has it been so ably supported, and the difficulties in the acceptance of the doctrine of the duality of the venereal poisons so forcibly put forward, as by Dr. Morgan. He believes that the sore which exhausts itself locally, and the sore which is but the first manifestation of a long chain of morbid phenomena acknowledge the same parent, and in the work before us he offers an explanation of the difference in the course of the one and the other. He says:—

“I cannot agree in the sharp distinctions that have been drawn. I am free to admit that I can say conscientiously and plainly to a patient suffering from a non-indurated sore, with well defined abrupt edges, secreting surface, and abundant pus formation, that he will *probably* escape constitutional signs, and that the majority of young healthy men who get this form, do escape them, yet I am careful to warn him that I cannot be certain or ensure him against their appearance. So far as prognosis, therefore, is concerned, or for clinical purposes, I am a

believer in the existence of a form of sore, presenting the characters of "the chancroid" in *the male*, which will probably exhaust itself locally, as a secreting and pustular sore, but it by no means follows that it is a distinct poison. It is, I believe, but a modification of the true infecting chancre, and has ever a tendency to show its original extraction by developing, in some instances, its true infecting power. In *woman*, on the other hand, this definition does not hold good; and in this is one of the difficulties which besets our coming to a certain appreciation of the nature of the primary disease."

The circumstance to which allusion is made in the last sentence is one on which Dr. Morgan relies very strongly. Among women admitted to the Lock Hospital, no matter what the character of the primary sore, constitutional contamination is the rule, not more than one in ten, according to the author's observations, escaping; in men, on the contrary, who must derive their infection from these very women, not more than one in three who get sores suffer constitutional contamination; reflecting on this fact Dr. Morgan was led to a conclusion which, if confirmed by other observers, will do more than any other modern discovery to clear away the difficulties in understanding venereal disease. He found that the vaginal discharge of women who, though free from sores in their genital organs, were suffering under constitutional syphilis, was capable of producing sores which had the characteristics of the soft or non-infecting sore, an observation which goes in the direction of showing that the poison which produces the soft is the same poison which causes the infecting sore, but modified by its passage through the system. This observation affords in addition an explanation of a fact which has puzzled many previous observers, the fact that while in hospital practice, and with the lower orders, the soft, simple, or non-contaminating sore, is most frequent, yet in private practice and among the higher ranks the indurated, or specific, or infecting sore is rather in the ascendant, a difference which Dr. Morgan believes is due to the circumstance that the women visited by the affluent are careful to use the syringe and astringent injections, and so, the vaginal discharge being washed away, they seldom give sores, except when they are actually the subjects of unhealed chancres, whereas the women to whom the lower orders go, being careless as to cleanliness, have their passages constantly bathed in vaginal discharge, capable of producing chancroids.

Having discussed this question and described the initial venereal sores, Dr. Morgan gives a full account of the various lesions

throughout the body by which they may be followed; he gives an admirable tabular scheme of the syphilitic rashes, and an account of mucous patches and vegetations, and of syphilitic diseases of the eye, the larynx, the viscera, and the nervous structures. He devotes considerable space to the last, and shows that paraplegia, locomotor ataxy, local paralysis, hemiplegia, and epilepsy may all be brought on by syphilitic disease of the membranes, of the brain and cord, or of the nerves. Originally discovered by Dr. Thomas Reade of Belfast, the existence of syphilitic lesions of the nervous centres has for many years been an established fact, the various anatomical changes which the poison can produce, have been thoroughly described by various writers, both in Britain and abroad, and, in consequence, many affections of the nervous system are successfully treated by mercury and iodide of potassium which were left uncured until the liability of the nervous structures to syphilitic change was pointed out by Dr. Reade.

Dr. Morgan gives full directions as to the treatment both of the primary sore and the various subsequent phenomena. His work should be read by all who are interested in venereal disease, and we believe it has been already ordered by the Director-General of the Navy to be supplied to the libraries of the Naval Hospitals.

Mr. Bradley also adopts the doctrine of the unity of the chancrous virus, supporting his opinion, among other grounds, on the fact that he has succeeded in producing soft sores in the lower animals by inoculation from a hard sore in the human being. He likewise contributes some interesting observations on rare syphilitic lesions, on syphilitic renal dropsy, and syphilitic post-partum hæmorrhage.

Dr. Drysdale too supports the essential unity of the non-infecting and the hard infecting chancre, and gives in the course of a single paper a very complete, though of course, a not very detailed sketch of the various lesions which may acknowledge syphilis as their cause.

Dr. Beaney's work is intended for popular use, does not profess to describe treatment, and owes any value it possesses to some plates which are copied from Ricord and Cullerier.

PART III.

HALF-YEARLY REPORTS.

REPORT ON SURGERY.*

By EDWARD H. BENNETT, M.D. and C.M., T.C.D., F.R.C.S.I., M.R.I.A. ; Surgeon to Sir P. Dun's Hospital ; University Anatomist and Lecturer on Clinical Surgery in the School of Physic.

THE compiler of the Report^b of Surgical Cases treated in the army of the United States from 1865 to 1871 states, in very forcible language, his approval of the opinion held by M. Legouest, as to the treatment of penetrating wounds of the abdomen.

The experience of Assistant-Surgeon Otis as a writer on military surgery is well known to all who have read the splendid reports issued by the American War Department since the late war, which have been compiled by him. His opinions are well entitled to command respect, and to influence the practice of surgery, derived as they are from the contemplation of such vast materials as the American army affords.

He says—"The wounds of the abdomen and pelvis include some remarkable examples of recovery from accidents of the gravest nature; but the general mortality is so very large, as to furnish an additional argument in behalf of M. Legouest's proposition to incise the abdominal walls, and explore the track of the projectile in certain gunshot penetrating or perforating wounds of the cavity. Thus, only in many cases, can the patient exchange the probability of inevitable death for the possibility of recovery, either through the prevention of extravasation by enterorrhaphy, or the bringing of the wounded viscus into apposition with the abdominal walls.

* The author of this Report, anxious that every contribution to surgical literature should be noticed, will be glad to receive any publications on surgical subjects. If sent to the correspondents of the Journal they will be forwarded.

^b War department, Surveyor-General's Office, Washington, August 17, 1871.—Circular No. 3. Report of Surgical Cases in the Army.

For one, I am free to assert that where there is evidence that internal hæmorrhage, or fœcal extravasation is going on, what may be termed the 'ostrich plan' of giving opium, and 'making the patient comfortable,' should be abandoned. And, I believe, that prejudices similar to those that ovariectomy has successfully overcome in the last quarter of a century will be dispelled by the results of exploratory incisions in gunshot wounds of the abdomen, before many years have elapsed." Without delaying to consider the meaning of the phrase, "the probability of inevitable death," we may examine the grounds of the opinion so confidently expressed by Assistant-Surgeon Otis in the foregoing passage:—

Of thirty-seven cases of penetrating wounds of the abdomen reported, thirty terminated fatally. Of these, the cases of wound of the small intestine were twenty; all of which were fatal, seven immediately, being complicated by wound of the large vessels; ten others died, mainly of hæmorrhage, within forty hours. Three survived three, five, and twenty-nine days respectively, and succumbed to peritonitis.

The reports of these or of the remaining cases contain no account of any exploratory incision having been made, the opium treatment, so strongly condemned, being the method adopted, so that we are forced to conclude that the recommendation expressed in the passage quoted above is not the result of any experience of the proposed treatment, but is simply deduced from *à priori* reasoning, supported by the authority of M. Legouest.

M. Legouest^a does not propose the exploration of the wound, with a view to the arrest of hæmorrhage, but only in dealing with the injury of the intestine, and to attempt the extraction of balls lodged in the abdominal cavity.

He says—"In cases of lesion of the intestine by cutting weapons, accompanied with extravasation of solid and liquid material, and in gunshot injuries, it is then proper to enlarge the external wound with the knife, to draw out the intestine, and to close by suture the wound in it."

With reference to balls lodged in the cavity of the peritoneum, M. Legouest says "that all surgeons who have written on gunshot injuries, with the exception of Bandens, agree in advising that no search should be made to discover a ball lost in the abdomen, and base their opinion on the danger attending the search, and on the

^a Legouest, *Traité de Chirurgie d'Armée*.

possibility of recovery taking place, in spite of the presence of the ball." . . . "This practice is in accord with that which interdicts the exploration of wounds of the abdominal cavity, and which would leave to the efforts of nature the cure of perforations of the intestines, when they are not protruded from the abdominal cavity."

He thinks with Bandens, in opposition to such a weight of authority, that it is necessary to search for balls so lodged, and for intestinal perforations caused by their entering into the cavity; being of opinion that the introduction of the finger into a penetrating gunshot wound of the abdomen does not add anything to the gravity of the injury, even when it fails to disclose the nature of the wound or the presence of the ball.

Assistant-Surgeon Otis would extend this treatment to all cases, either those of hæmorrhage or of wounded intestine. When we consider how few cases of penetrating gunshot wound of the abdomen occur, in which the injury of the intestine, or of the vessels, is such as to admit of their being discovered and effectually dealt with, we are led by the same method of reasoning as that of the author of the report before us, to conclusions the opposite of his. Nor do we think it probable that our opinion is likely to be altered by any experience derived from the adoption of the treatment. The surgeon must indeed be sanguine who hopes to be able to close efficiently, even supposing that he could find them, the many wounds of intestine and vessels caused by the passage of a ball through the abdominal cavity. Any one who has experienced the difficulty of finding the openings made either in the intestine or in the blood-vessels of the abdomen, by disease or injury, under all the advantages afforded by the practice of the dead room, with a fine, free, crucial incision of the abdominal walls, will be slow to search for these with a finger introduced through the wound, or even by the more extended incision of ovariectomy. In his report of work done under the red cross, in the last European campaign, Mr. MacCormac expresses the opinion of himself and his colleagues on this subject:—"We were not tempted, as was suggested by an eminent German professor, to perform an operation, as if for ovariectomy, to find out the perforated stomach, intestines, or liver, as the case may be, sew the wounds up, clean out the cavity of all clots and foreign substances, close the external wound, and then hope for a favourable result. I fear the want of such a success will have to be deferred for a very long time."^a Ledran says of this practice—that

^a Notes and Recollections of an Ambulance Surgeon.

"it is unnecessary to *amuse* oneself in search for a ball lost in the cavity of the abdomen." However, the authority of advice contained in the report of surgical cases, published for the information and instruction of medical officers of the American army, may have its effect, and we may have, in some future issue, records of the results of this practice.

Four cases of excision of the upper end of the femur are recorded in the present report, of which three were successful, two of the patients retaining very useful limbs. The reporter very justly holds that these results place excision of the hip for gunshot injury among the established operations of surgery. The examination of the reports of the cases leaves no doubt that the operations were not only admissible, but that they were necessary; no other treatment but amputation at the hip, or the abandonment of the patients to a rapid death, being possible. These fortunate results diminish the death-rate following the operation for wounds of the femur to 91·9 per cent., a death-rate fearful to contemplate, but still preferable to that of amputation at the hip for gunshot injury.

Mr. Furneaux Jordan^a holds that all ulcers are due to constitutional causes, and that of constitutional causes there are but three—syphilis, eczema, and struma. "All ulcers are syphilitic or eczematous or strumous. The presence of syphilis is the essential condition of the origin and existence of one and the largest class of ulcers. If syphilis became extinct the largest class of ulcers would also disappear. If eczema ceased to exist, these also would cease to exist. Struma is essential to the existence of another, but a much smaller class. I have thus, by anticipation, replied to the objection that syphilis, and eczema, and struma, are complications like indolence or inflammation, or hæmorrhage, or senility." Again, "excluding the small number of strumous ulcers; and the extremely small number of strumous ulcers on the legs of adults, I never see an ulcer which is not either syphilitic or eczematous, nay more, which is not more syphilitic than indolent, or varicose, or inflamed, or which is not more eczematous than indolent, or weak or neuralgic." Again, "the ulcers which occur on the legs of adults, are, as I have already stated, either syphilitic or eczematous." The apparent originality of these statements disappears when we consider the following quotation from the author's paper,

^a Birmingham Medical Review, No. 1.

which shows that the statements of the author differ from those current in every text book merely in their logic. "I speak of ulcers properly so called; of those diseases in which a circumscribed loss of integument (its whole thickness) is the principal condition. There are many ulcerations which are not, and correctly so, classed as ulcers; such for instance, are those which follow operations, injurious abscesses, carbuncles, burns, escharotics, gout (with concretions), scurvy, chilblains, cancer, skin disease, &c., &c. These ulcerations are secondary, and might not inaptly be called contingent ulcers." Yet the author tempts us to read his paper by stating that his "object is to show, what has been before in some degree suspected of many, that all ulcers are the result of diathetic or constitutional states."

Dr. Hood^a applies himself to the task of placing before the profession a clear account of the methods, failures, and successes of the so-called bone-setting. He defines bone-setting to be the "art of overcoming, by sudden flexion or extension, any impediments to the free motion of joints that may be left behind after the subsidence of the early symptoms of disease or injury; perhaps, indeed, more frequently of the latter than of the former." This definition is the result of knowledge derived from an intimate acquaintance with the practice of a recognized bone-setter. The success of the bone-setter depends in point of fact upon the frequent occurrence of what may be called a minor degree of false ankylosis variously produced, and perhaps located in different natural or adventitious structures, at least such is the opinion of Dr. Hood stated in his chapter on pathology.

Although Dr. Hood devotes a long chapter to the subject of the pathology of articular disease, we have failed to discover any evidence of his practical acquaintance with the subject on which he writes. No reference could be more unfortunate than one which he makes to the work of Sir A. Cooper, almost the only definite pathological reference made in his work—"The joints most prone to partial dislocation from injury are probably those of the tarsus and wrist. Sir Astley Cooper mentions also the ankle, knee, shoulder, and elbow, as being liable to this form of accident, and gives an account of the anatomical conditions in a partial dislocation of the humerus found in a subject dissected at St. Thomas's Hospital in 1819."

^a Bone Setting (so-called) by W. P. Hood, M.D., &c.

Dr. Hood does not seem to be aware of the fact that the case to which he refers has long since (as long ago as 1836), been proved to be an instance of a common and well known disease of the joints, chronic rheumatic arthritis, not a subluxation the result of injury. For information on the subject we may refer him to the paper published in this Journal in 1853, by Dr. R. W. Smith, and to Mr. Adams's paper, on the subject in the *Cyclopædia of Anatomy*; and again, to his more recent work published in 1857.

Dr. Hood holds that the profession has neglected the proper study of diseases of the spinal cord and of the spine. He says, "If the profession generally would so study the diseases of the spinal cord as to rescue them from specialists, the first step would be taken towards rescuing the diseases of the vertebral column from quacks."

"The surgeon who is consulted about a case of spinal malady should first of all make sure that he is not frightened by a bugbear, and should then proceed to determine by scientific methods of examination, whether or not he is in the presence of disease of the nervous centres, or of caries, abscess, or other destructive change in the vertebral column. On such points as these no man who possesses a thermometer, a microscope, and test tube, has any excuse for remaining long in doubt, and if he is able to exclude the possibility of such conditions, he may then regard the spine simply as a portion of the skeleton, and may deal with it accordingly." That is, give it "the preliminary twist," and then "the important thumb pressure" in which his treatment consists chiefly. Dr. Hood has a low opinion of the acquired knowledge of the profession, and suggests indeed a ready, rapid, and certain remedy—yet he seems to think something more is required, possibly his own assistance in these cases—for he closes his valuable work thus:—"The diagnosis must be made in each instance for itself, and in each must depend upon the sagacity and the skill of the practitioner."

Sir James Paget^a records a series of cases for the purpose of proving "the sufficiency and safety of removing innocent tumours of whatever kinds from bones. The sufficiency is established by the times that have elapsed since the operations without any occurrence of disease—times which, in even the shortest cases, are more than would have passed if recurrence should have happened

^a *Medico-Chirurgical Transactions*, Vol. liv.

in consequence of incomplete removal of disease; and I have seen no reason to doubt that the safety of removing a tumour from within a bone is greater than that of any resection or amputation that might have been performed, as an alternative operation."

"The only difficulty is to make a correct diagnosis of cases for which the different operations for the removal of tumours growing in or on bones are severally appropriate. For cancerous and recurrent tumours, amputation or wide resection is generally advisable; for innocent tumours growing in bones, enucleation."

For help to the diagnosis necessary for choosing the right operation for bone-tumours, I venture to offer these conclusions from many observations:—

"1. The tumour is probably cancerous if its growth commenced before puberty or after middle age, unless it be a cartilaginous or bony tumour on a finger or toe, or near an articulation.

"2. If a tumour has existed on or in a bone for two or more years, and is still of doubtful nature, it is probably not cancerous or recurrent, and this probability increases with the increasing duration of the tumour.

"3. If a tumour on or in a bone has doubled or more than doubled its size in six months, and is not inflamed, it is probably cancerous or recurrent, and this probability is increased if, among the usual coincidences of rapid growth, the veins over the tumour have much enlarged, or the tumour have protruded far through ulcerated openings and bleeds, and profusely discharges ichor.

"4. If with any such tumour, not being inflamed, the lymph glands near it are enlarged, it is probably cancerous, and still more probably if the patient have lost weight and strength to amounts more than proportionate to the damage of health by pain or fever, or other accident of the tumour.

"5. A tumour on the shaft of any bone but a phalanx is rarely innocent, and so are any but cartilaginous outgrowths on the pelvis, or any but the bony tumours on the bones of the skull."

On the concurrence of these characters, for in these, as in all other instances, there is no one sign which can be always or absolutely relied on for diagnosis, Sir J. Paget relies for the formation of a correct diagnosis of the nature of a bony tumour previous to the undertaking of any operation for its removal. "When these means of diagnosis are insufficient, an exploratory puncture or incision may be made; and generally, in cases of doubt, it may be well to attempt the simple removal of the

tumour, and in case of failure to proceed at once to its excision, or to resection or amputation."

The following cases are of interest, as evidence that the subcutaneous pneumatic aspirator of Dieulefoy can be used with safety and advantage in the treatment of retention of urine:—The pathological investigation recorded in the first is the only observation of the kind we have as yet seen, and is the chief interest of the case, for it shows that the instrument, so far from inflicting any serious injury on the bladder or the surrounding tissues, may leave little or no trace of its passage. The statement of Dr. Dieulefoy,^a "that it is always possible, by means of subcutaneous pneumatic aspiration, to explore, without danger, for any collection of fluid, whatever be its seat or nature," cannot be accepted. Indeed, so sweeping an assertion seems only calculated to excite distrust of the author and of his apparatus in the mind of any reasonable person. We are aware that, in one group of cases, the use of the instrument has been attempted with anything but freedom from danger—namely, in hydrops articuli; while in others it has failed even to give satisfactory results as a method of exploration, in consequence of its tube becoming at once blocked by the semi-solid contents of certain cysts or abscesses. Hence, we look to reports of cases in which it was employed to judge of its merits, rather than accept the statement of its author, without reserve. On 4th January, 1871, a mason, who had fallen from a scaffold an hour previously, was admitted to the Hospital St. Antoine, under the care of M. L. Labbé.^b He lighted, from a height of six or seven feet, straddling on a beam, and was much bruised in the perinæum and anterior part of the abdomen. On the following morning, a considerable extravasation of blood was found, extending from the spine to the scrotum. The patient complained of severe pain in the abdomen, which was very sensitive to the touch. The skin was hot, and the pulse frequent—120. A poultice sprinkled with laudanum was applied to the abdomen. Since the accident, the patient had been unable to pass water, in spite of frequent efforts, and the distended bladder reached to three fingers' breadth below the umbilicus. Almost immediately after the accident, about a spoonful of blood had flowed from the urethra. Catheterism having been tried, without success, by the resident, M. Labbé attempted to pass instruments of various sizes, but failed. He

^a On Subcutaneous Pneumatic Aspiration, &c., page 15.

^b Journal de Médecine et Chirurgie, 1871, page 542.

prescribed a prolonged warm bath, and finally incised the perinæum without success. He then, the necessity of relieving the distention of the bladder being urgent, punctured the viscus with the long No. 1 needle of the aspirator above the pubis. The moment the needle entered the bladder, the urine flowed freely into the vacuum. The needle was sunk more deeply as the bladder emptied itself, and so about fourteen ounces of urine were drawn off. The puncture caused no pain, beyond the pricking of the skin. Several times, at various intervals, the operation was repeated, at points near the first, and always successfully. Unfortunately, the patient was unable to bear up against the peritonitis resulting from the fall, and died, so affording an opportunity of examining the effects of the operation.

The outer surface of the bladder presented on its outer part little red ecchymotic spots, like flea bites on the skin. At this part the punctures had been made. On the inner surface of the bladder, it was not possible to find the least trace of the passage of the needle. The urine contained in the bladder was perfectly normal, and it contained neither blood, nor clots, nor pus.

In February, 1870, M. Labbé was sent for to see a man, sixty-five years of age, suffering from complete retention of urine. The old man had a very large prostate, and various attempts to pass a catheter had been made for two days without success. These efforts were not completely harmless, for the patient had lost some blood from the urethra, and suffered much pain.

M. Labbé abandoned the idea of a further attempt at catheterism, and considering the enormous distension of the bladder, and alarming condition of the patient, decided on puncturing the organ. For this purpose, he used the aspirator. No. 2 needle was introduced a little above the pubis, and about seventeen ounces of urine were drawn off. The relief was immediate. Two hours later, a full-sized catheter was readily passed; all trouble ceased; it was unnecessary to interfere further, and the patient suffered neither any pain or further accident from the operation.

These cases show that for the relief of retention of urine, the instrument seems most perfect. In this proceeding, there does not appear to be any reason to fear that the operation will prove unsuccessful, in consequence of the blocking of the tube used, as occurs in dealing with certain abscesses.

PART IV.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

Wednesday, January 24th, 1872.

DR. LAW, in the Chair.

THE CHAIRMAN said:—I thank you for asking me to take the chair on this very interesting occasion. I should willingly have declined to do so if I could, because I did not happen to be present during the reading of Dr. Burke's paper. As the discussion is to be confined solely to his paper, perhaps he will be good enough to repeat its principal features. We all have great reason to be obliged to Dr. Burke for having brought forward so interesting a subject. We all remember how interesting was the meeting at which we had the discussion on the subject of cerebro-spinal meningitis. The present is also a subject of absorbing interest.

DR. BURKE.—As you were not here on the last night, Sir, I propose to bring before the meeting some of the most salient features of the subject under consideration. On the last occasion I read my remarks, not because I had anything new to communicate, but in order to elicit from those who have had the most experience their opinions as to what is to be done in the present emergency. Dr. Burke then repeated some of the principal parts of his paper, and afterwards proceeded to say:—We have got the outbreak here now; and the question is, how can we prevent it from spreading? I must say that the only action that has been taken in the matter has been taken by his Eminence, Cardinal Cullen, in preventing wakes. Scarcely anything has been done by bodies in authority, such as corporations, boards of guardians, &c. The wakes, without the slightest doubt, spread the small-pox. Convalescents from that disease cannot be retained long in the hospitals, for obvious reasons. In the first place, an hospital is a very bad place for convalescents. In the next

place, there are always urgent cases seeking admission, and you must get rid of the chronic cases, in order to make room for the more acute. At the present moment there is not sufficient accommodation for the conveyance of small-pox patients to and from the hospitals. There is, as yet, no convalescent home anywhere. Mention was made of a place adjoining one of the prisons; but to bring the disease up close to the poor creatures confined there would be a very hard measure. The best thing to do would be to take a number of houses in different places for convalescent homes. There are many vacant houses about Dublin suitable for the purpose. Another question which has been frequently asked me, and which I have not been able to answer, although I trust that some of our distinguished members present may be able to throw some light on it, is:—When is a person free from infection after small-pox? Of course there is no doubt about the protective power of vaccination. We have information as to that from every side. Dr. Grimshaw will tell you how high the per-centage of deaths of unvaccinated patients has been in Cork-street Hospital. The question as to re-vaccination has crept up. Though my opinion is that if there be a good mark on the arm, the protective power of vaccination does not diminish with years; yet when I find such men as Mr. Simon, of the Privy Council, and Dr. Beatty, whose experience is so large, advocating re-vaccination, I think we should not refuse to re-vaccinate any applicant.

The CHAIRMAN.—We hope that our visitors will give their opinions as to whether re-vaccination should be adopted or not. The fact of the occurrence of small-pox after vaccination has been noticed as much and as generally in France as here. Trousseau noticed it. In the beginning of this century scarcely ever a case of small-pox occurred. If it did, the question was afterwards raised as to whether or not vaccination had been properly performed. But during the last thirty years, cases of small-pox have scarcely ever been wanting, and they could not but connect it with the change in the character of the disease through which we have been passing during that period. It was also said in our own country, about twenty years ago, that whenever small-pox occurred it was in consequence of vaccination not having been properly performed. The fallacy of that view might have at once appeared from the fact that a modification of the disease appeared even where it had been properly performed. The point now is, as to whether re-vaccination is to be performed or not. In the Prussian Army it is performed every seventh year.

DR. GRIMSHAW.—As Dr. Burke has called on me in reference to the hospital with which I have the honour to be connected, at the risk of being troublesome I will say a few words. I cannot do better than give

a summary of how the numbers have stood up to the present, in Cork-street Hospital, as to small-pox. Some of the first cases that appeared in Dublin were admitted there, though we did not receive the very first cases. Up to the present, the number of cases admitted into Cork-street Hospital has been 281. The number treated to a termination 223, and of those at present in the house none are likely to prove fatal. The number of deaths has been 56, being a per-centage of 25·1 on those treated to a termination. Of the total number 202 had been vaccinated. Of these 19 died, being 9·4 per cent. In 3 of these cases we had to rely on the statements of the patients, as they were so covered with small-pox when they came in that we could not discover any marks of vaccination upon them. Three others were undoubtedly vaccinated in the presence of small-pox, and perhaps had taken the disease before they were vaccinated. There was one remarkable case of a person who was vaccinated on Saturday morning, and on the same night that person shivered, had pain in the back, and sickness on the following day, and two days afterwards was admitted into Cork-street Hospital. If we separate these cases from the rest we have a mortality, in the vaccinated cases, of 7·9 per cent. With respect to the unvaccinated cases, they gave 34 deaths out of the 56, being 60·77 per cent. The contrast between the two sets of figures is very remarkable. Of doubtful cases we had three, all of whom died. They said they had been vaccinated, which I did not believe, not having marks on their arms. As to the introduction of the disease into Dublin, Dr. Burke has referred specially to some cases reported to the Registrar-General. Of the first 15 cases admitted into Cork-street Hospital, 8 were distinctly traceable to England. The first of those was one of the cases referred to by Dr. Burke, where the clothes of a man who died of small-pox in England were brought into that wretched labyrinth of courts and alleys at the south side of Kevin-street. Shortly afterwards a woman nearly nine months pregnant was attacked with small-pox. She aborted, and the child died and she died. Two other persons were admitted into Cork-street, from the same house. Three others were attacked in the house, and one was removed to another hospital. One died and one recovered in the house. From that locality we had numbers of cases admitted into the hospital. This—if nothing else had occurred in Dublin—would have been sufficient to start the disease on the south side of the City. The second case that came into Cork-street Hospital had had vomiting and pain in the back before he started from England. The fourth case came from Drogheda, where she had been residing with an English family, amongst whom there had been small-pox, they having recently come over from England, and engaged her as a temporary servant. The seventh case had come over to work in the cooperage of Messrs. Guinness's brewery, and was sent by Dr. Jennings to the hospital. In five or six days afterwards two more

cases were admitted from Guinness's cooperage. Ten or twelve cases altogether occurred among the persons employed in this cooperage, and I believe a large number occurred amongst the employees of that establishment. These may be almost altogether traced to the unfortunate fellow who came from England with small-pox. The ninth case was only fourteen days from England. The eleventh case came from Crewe, and was attacked in a more ferocious manner by the disease than any of the others that I have mentioned. He was an artisan in railway work at Crewe, and came to work at Inchicore. Two of his family—boys—were seized with small-pox. One while he had it went to Cork, and propagated the disease there. The other came to Cork-street Hospital. The twelfth case was imported from Liverpool. These were all the cases that we traced to England, and they were all amongst the first 15 cases that were brought to the hospital. With regard to the other institution with which I am connected, namely, Steevens' Hospital, I shall give you the details of a return, made up to last Saturday night. There were 28 cases admitted. Of these, 2 died of the disease. One of these was unvaccinated, being the only unvaccinated case in the 28. Another was a wretched scrofulous patient who had been in the hospital three months before with *necrosis* of the tibia, from which he recovered. He was one of the 27 vaccinated cases, and he died of the disease. Another case died in that hospital, but it is questionable whether or not his death can be attributed to small-pox. He had phthisis before he was admitted. He came in with small-pox. He recovered from the small-pox, which was in a very much modified form, and died of tubercular meningitis. A *post-mortem* examination left no question as to the cause of his death. Adding these cases together for the two hospitals—all of whom I saw, and more than half of whom I had under my charge—there is a total of 289, with 58 deaths, or 20 per cent. Of the vaccinated cases, numbering 229 amongst the two hospitals, there were 20 deaths, or 8·7 per cent., less the few cases I have mentioned, which were doubtful, and which had been vaccinated in the presence of the disease, which would reduce the mortality for these to 6·1 per cent., for the vaccinated cases. As Dr. Burke has remarked, it is a matter of regret that some similar precautions—short of slaughtering the patients—had not been taken with regard to small-pox, as those which were adopted in the case of the cattle plague. Not only should precautions have been taken to prevent the disease from getting into the country, but after it got in, measures should have been taken to restrict it to certain districts. None of those precautions have been taken anywhere in Ireland, not even in Dublin. In a house in Ardee-street a case of small-pox occurred, which was brought under the notice of the authorities, not only by myself, but by others. The patient was brought to the hospital and died. Then followed a

great number of cases—I cannot tell you how many—in the same house. Four of these were admitted into Cork-street, two of whom died; and I have little doubt that if proper arrangements had been made in this instance the disease would have been stopped at that point at least. This not only shows the defect of special arrangements with regard to small-pox, but also the defect in our whole system for the prevention of disease, for this house was well known as one in which all kinds of contagious disease had been prevalent from time to time. It was attacked during the cholera epidemic, and was well known as a fever house. It was not watched. If it had been, the disease might have been stopped in that street, instead of having spread into Mill-street and New-market, and all the immediate neighbourhood. The spread of the disease for want of ordinary precautions has been something terrible, and the neglect of informing the authorities on the subject has been very culpable. For instance, in a hotel in this street a case of small-pox occurred. It was hushed up. I am informed that two people left the hotel and afterwards got small-pox, but cannot answer for the accuracy of the statement. I have also heard of instances of people working in tailors' workshops with the disease on them. I have at present one of those cases; the man was working for his master until yesterday, and to-day he is covered with small-pox. Another patient, who has been admitted into hospital, was dressing ladies' hair in Dawson-street until yesterday, and he is now covered with spots. I have a butcher in the hospital who cut up four carcasses of beef after he had the pimples on him. I have a grocer who was attacked in his shop, and sold tea and sugar to the public with the rash on him. I have a telegraph clerk, who was working in the Post Office after the eruption was out on him. He noticed the rash coming out while he was in the office, and left it. With such things as these occurring it is no wonder that the disease should be spreading in every direction. With reference to re-vaccination, there is one point worth mentioning. We have here somewhere about 300 cases. Not one of these cases had been re-vaccinated, although the number of re-vaccinated cases is very small compared with those who had been but once vaccinated, yet the absence of a re-vaccinated case in 300 is a significant fact. I have one patient who had small-pox a second time. I would wish to call attention to a class of what I would call abortive cases. I have seen a number of these cases, in which the temperature rose to 103; in others it was not above 100. These patients had intense pain in the back, vomiting, and all the symptoms of small-pox, except the rash. One of these persons had small-pox before, and had been residing in a house where small-pox was. I had myself a similar attack, but whether it was small-pox or not, I am not prepared to say. I was never successfully vaccinated. It was attempted nine times with me, and was always unsuccessful; and whether I am susceptible of small-

pox or not I cannot tell. I was informed by medical friends that they had met with similar instances recently, and that they had not noticed them previous to the present small-pox epidemic. I wish to call attention to one point in connexion with the prevention of the spread of the disease. There has been a great deal of talk about the restriction of communication between small-pox hospitals and people outside. It has been proposed for instance that we should destroy the privilege of patients being visited by their friends in the hospital. It has even been suggested—though the suggestion is perfectly ridiculous—that we should refuse to give up the bodies of those who had died of small-pox. The effect of any such restrictions would be to diminish the admissions into the hospitals, and keep the patients at home, for with such a state of things, and the prospect of being imprisoned for three, four, or five weeks, they would not come to the hospitals. My impression is that the amount of injury that may be done by allowing the patients' friends to visit them, and even the amount of injury that is done by bringing small-pox patients to the hospitals in common cabs is less than the damage that would accrue by throwing obstacles in the way of the patients coming to hospital (hear, hear). Those recommendations, and several others of the sort, some of them made by members of our own profession, and some coming from the Public Health Committee, are in my opinion very mistaken. If we had all proper arrangements complete, and everything in apple-pie order—which we have not—some of these arrangements might be enforced with certain limitations; but at present I do not think that they can. The main point is that mentioned by Dr. Burke, of providing proper refuges for convalescent patients, so as not to compel them to go directly home from hospital. While on this point of convalescent homes, I may refer to another of more importance, namely, the establishing in all large towns of refuges, not for patients that are sick, but for persons who have been in the presence of contagious disease, and who are liable to catch the contagion thereof. The authorities have the power of establishing these under the Sanitary Act of 1866. That is, supposing that small-pox breaks out in a common lodging-house, or a house set in tenements, that house should be cleared out at once, and all the healthy people in it should be removed to the refuges and kept there till they are safe; the sick should be sent to the hospital. Closing an infected house is of no use unless the people in it are removed to a place where they will be safe until all chance of their getting the disease has passed away.

DR. DENHAM.—I should like to have some further information with regard to the man mentioned by Dr. Grimshaw as having come to Guinness's brewery. I should like to know how long that man was from England before he entered Guinness's brewery; how long he was in the brewery before the disease appeared; how long he remained after the

symptoms were developed; how soon after the symptoms were developed, and he was removed to hospital. Persons in the brewery seemed to have got the contagion from him. That case is of vital importance, and the most tangible that Dr. Grimshaw has brought forward.

DR. GRIMSHAW.—That patient went to work in Guinness's brewery the day after he came over. He was in Cork-street Hospital in five days after his arrival from England. He only worked two days in the brewery.

DR. DENHAM.—How soon afterwards were the three patients attacked?

DR. GRIMSHAW.—He was working with the eruption on him, and during the following three weeks I cannot tell you how many cases were admitted from that brewery; but the first case was admitted within ten days after he was admitted. I could not tell you the date at which the next case followed. This was certainly a case of contagion.

DR. HAYDEN.—It would be well if the Society were to address itself strictly to the matter within the scope of Dr. Burke's paper. No doubt my friend, in travelling beyond its limits, has given us a great deal of valuable information; but I hope the time will arrive when the Society will have placed before it, in a statistical form, the whole experience of the physicians of Dublin with regard to this epidemic. It seems to me much better on the present occasion to consider the very important matters which Dr. Burke has submitted to us—viz., first, with regard to the preventibility of small-pox; secondly, with regard to the mode of conveyance of patients from their homes to the hospitals; thirdly, as to the time when it is safe to discharge a patient from the hospital; and fourthly, as to the value of vaccination and re-vaccination as means of preventing small-pox. First, as to the best means of preventing the spread of the disease when it has entered the habitations of the poor, I will not venture to offer any suggestions on the subject. Indeed it seems to me that the rules which apply to the prevention of all other contagious diseases apply pre-eminently here. I have seen myself in the habitations of the poor, on more than one occasion, where the mother was sick, the children of the house, who may or may not have been vaccinated, round the mother's bed. I have seen the husband performing the small offices of nurse, and the neighbours surrounding the bed, and seeming to have no dread whatever of the disease. That is a melancholy state of things. It is extraordinary, with the experience that the poor of Dublin have—the melancholy experience—of fatal epidemics, such as cholera and purpuric cerebro-spinal meningitis, that they have not a dread of this hideous disease, which is so pre-eminently repulsive in all its features.

Dr. Burke has suggested one very important explanation of this state of things, and I have no doubt that he is strictly correct when he says that the very name of the "sheds" has a most deterrent effect on the poor. The name "sheds" with the poor means a sort of extemporized structure, put up more for the purpose of conducting them to eternity than of restoring them to health. That is their impression at least. As I shall have occasion to show in two or three minutes, if you will bear with me, there is the strongest possible reason for dissociating small-pox from other diseases in the hospitals in which they are treated. As to the mode of conveyance of small-pox patients to the hospital, I think the arrangements hit on by the Corporation are eminently ludicrous. You are all aware that there is only one mode of conveyance, and that is by means of cabs. When a person is stricken with the disease the first impulse of his friends is to send for a cab. Now we all know that cabs thus used are a means of communicating and extending the disease. When the patient is put into the cab, the windows are shut, and all the upholstery of the cab becomes saturated with the *miasma* of the disease; and the person who next steps into the cab gets the full benefit of that exhalation. The Corporation propose, as a remedy for this, that a policeman shall be placed in front of each hospital for the reception of small-pox patients, whose duty shall be to take possession of every cab that comes with a small-pox patient, and detain it for twenty-four hours, for the purpose of being disinfected. What is the meaning of that? It simply means that the poor cabman who accepts a fare of sixpence for taking a small-pox patient to the hospital runs the risk by so doing of losing twenty-four hours' wages. The consequence of that is, that he steers a medium course. He accepts the fare, and what does he do? I have known this to occur. He turns the patient out of the cab at the corner of the next street to the hospital; the patient walks to the hospital, and the policeman is bilked. He sees nothing; and the cab turns back and opens its door to the next passenger. This has occurred repeatedly; and I have seen the poor policeman standing day after day at the hospital door without anything whatever to do. And it seems to me that nobody has yet discovered the simplicity of this scheme. What is the plain remedy? As a matter of common sense, in a formidable epidemic like the present, the plain duty of the Corporation or the Poor Law Guardians would have been to see that every hospital that receives small-pox patients had a cab at its sole and exclusive service. That cab should be at the service of the hospital day and night; it should be duly noticed to the public as the only cab proper to be sent for on such occasions; and at the end of the epidemic one twenty-four hours' disinfection would suffice for it. As to the time when, in regard to the danger of spreading the disease by infection, it is safe to discharge a small-pox patient from hospital, it is very difficult indeed to draw the line; but it seems to me

that it is perfectly safe to do so when the process of decrustation has been completed. We know that the scales, and crusts, and scabs visible on the surface of the skin are the *debris* of the small-pox. I have no doubt that if an experiment were made it would be found that every particle of this matter is capable of communicating the disease. I would give the patient a warm bath, and a thorough washing and scouring; and when the scales had disappeared, then, and not till then, would I send the patient out—and I never heard of a case where the disease was propagated after that had been done. On the subject of vaccination and re-vaccination I have very little to offer. Before coming here I made a careful statistical return of the cases that had come under my own observation, most of them having been cases under my own immediate care. I shall give you briefly the result, which will furnish all the information that I have to offer. The total of cases that I have seen, and most of which I have treated myself, is 53. Of these cases I found—and I made it a rule in every case to ascertain the state of the arm with regard to vaccination—that 35 had good marks, that 8 had questionable marks, and that 8 had no marks at all. Of the 53 there were in all 24 severe cases; and what I mean by a severe case is, where the face was swollen, where there was delirium, where there was some difficulty in swallowing, where the anterior *nares* were blocked up—where, in fact, the disease was confluent. Of these 24 severe cases I found that 8, or one-third of them, had never been vaccinated; that 6, or one-fourth, had been imperfectly vaccinated; and that 10 had been successfully vaccinated. The total of deaths out of the 53 cases was 7. Of these 7 I found that 4 had not been vaccinated—that is to say, one-half of the total of unvaccinated cases died. I found that 3 of the 7 persons who died had been vaccinated—that is to say, that of the total of vaccinated cases one-twelfth, or a little more died. Now I think that this fact alone is in itself sufficiently striking to fix the attention of the profession. One half of the fatal cases had not been vaccinated at all, and only one-twelfth of the vaccinated cases died. As regards re-vaccination my experience does not go far. I have seen only one case that had been re-vaccinated contract the disease, and that was the case of a gentleman who had been re-vaccinated three weeks previously. At the end of the three weeks, when the vesicle had dried up, he manifested a few pustules on the face. He was scarcely sick; and but for the presence of the pustules I could not regard it at all as a case of small-pox. I saw no other case in which variola appeared after re-vaccination. My impression of re-vaccination is, that it is a most valuable means of preventing the disease; and as a proof of the force of my conviction I may mention that I have had myself and every person in my house, young and old, re-vaccinated. They all took the disease, some paying the penalty of very great swelling and tenderness in the arm; but if there were nothing

else to recommend it but the confidence with which it inspires people in the midst of a formidable epidemic, I think it is on that account a practice worthy of adoption. Irrespective of that, I think it is a most valuable means of prevention. There is one circumstance to which I wish to call the attention of the Society, and that is the displacing action of small-pox in regard to typhus and typhoid fevers. My own experience is, that I have not had a single fever case for the last three weeks in hospital. In other hospitals also there has been a marked falling off of fever cases. It is not easy to explain this, but it is at least a coincidence worthy of notice. It may be that small-pox has simply consumed the pulvum of these fevers.

DR. KIDD.—It seems to me that this question of re-vaccination is the most important part of the subject before us to-night. It will no doubt be in the recollection of the members of the Society that the subject was very extensively discussed at the Surgical Society during the last winter session. On two nights the members of the Surgical Society debated this subject of re-vaccination; and the present debate has this very remarkable and, I think, very interesting feature in connexion with it, that one of the gentlemen who spoke, perhaps, most influentially against re-vaccination, read his recantation here at our last meeting (hear, hear). For my own part I must say I was greatly gratified when I found that Dr. Beatty, whose influence and authority certainly carried great weight at the debate in the Surgical Society, announced here, and rose with the special object of announcing—fearing that he would not be able to be present here to-night—that he had so far changed his opinion that he now re-vaccinates every one who consults him on the subject. Now, we have discussed that subject, and have, I think, obtained as much information on it as in the present state of affairs we are likely to do. I question if we have yet any further facts of importance to bring forward. At the debate in the Surgical Society, when the epidemic of small-pox was spoken of as occurring in communities, and that as soon as re-vaccination was put in force the epidemic ceased, the reply was at once made—I think by a gentleman now present, if I am not mistaken—that the receptivity of the disease was exhausted. That argument, I think, stops all further debate. I don't know if there is any possibility of replying to it, at least on theoretical grounds. My object in rising to-night is to make a suggestion to this Society. I believe that individual experience on the subject—the experiences of hospital physicians, even, if we had them most accurately recorded, as I have no doubt they will be—teach us very little. If we are to consider the value of vaccination and re-vaccination, we must try and ascertain the number of persons at present living in the community who were vaccinated, and the number of these who have taken small-pox; the number of persons who have been re-

vaccinated and the number of these who have taken small-pox, as well as the number of unprotected persons and the number of these who have been attacked with the disease. This may seem a very huge undertaking; but it has been done before, and I think if this College were to apply themselves to it and to lend their weight and influence to it, it could be done again. There was an epidemic of small-pox in Newark-on-Trent in the year 1839. On that occasion Dr. Bigsby undertook a task of that description, and made a census of the east ward in Newark-on-Trent. He examined every individual in the ward except 48. There were nearly 4,000 persons living in the ward. He obtained returns of the numbers of persons who had been vaccinated, and who had been inoculated, and who had never been in any way protected from small-pox; and he has published these returns. I think that if this College were to set such a movement afloat, and were to endeavour to obtain similar returns, which I believe would be possible, although the undertaking would be expensive and arduous, it would be the only means of really setting this question of the value of vaccination and re-vaccination at rest. Obtain a return of the number of persons in Dublin who have been vaccinated and have taken small-pox, and a return of the number of persons who have taken it after re-vaccination, and we will then know the value of vaccination and re-vaccination. I know of no other way of arriving at that result. It is remarkable that Dr. Bigsby obtained nearly the same return that the Academy of Medicine of Paris obtained during the epidemic of Marseilles. There was an epidemic in Marseilles in the year 1825, and the Academy of Medicine did then what I take the liberty of suggesting that this College should do now. They had a census taken of Marseilles, and they found that of the vaccinated persons living in that town at the time of the epidemic, 6·6 per cent. took small-pox. Dr. Bigsby found that of vaccinated persons living in the east ward of Newark-on-Trent, 5·01 per cent. took small-pox. This, in my opinion, sufficiently shows the liability of vaccinated persons to small-pox. We had at the Surgical Society an instance mentioned of a ship at sea on board which small-pox broke out, and where there was no possibility of communicating with the shore, and there was a very confined community; and in that case from 10 to 12 per cent. of the vaccinated took small-pox. So that I think our present knowledge amounts to this, that under ordinary circumstances from 5 to 6 per cent. of the vaccinated will take small-pox; and that in a very limited community, where the exposure is very great, it may amount to from 10 to 12 per cent. Now, the instances of the disease being stopped by re-vaccination are so numerous, that it is quite unnecessary, I am sure, that I should attempt to detain the Society with them. I think, perhaps, the most interesting of them all, and the most important, is the one mentioned by Dr. Marston, where, on visiting a school for deaf and

dumb in which small-pox had broken out, he vaccinated the whole community. I think there were nearly 300 inmates in this school of the ages of from 10 to 15, which we know to be the age at which small-pox is most liable to occur. After he had re-vaccinated 261 persons, with the exception of one case that occurred some days after they were re-vaccinated, no further outbreak of small-pox occurred in the school. Such an example as this, I know, is met by the reply that all those who were capable of receiving the infection had taken it, and that that was the reason why the disease stopped. I really don't know any reply that can be made to such an objection as that.

THE CHAIRMAN.—I may mention that in the Nurses' Institution in Dublin, twelve nurses are kept constantly engaged in attending on small-pox cases. Of these twelve only one had not been re-vaccinated, and she was the only one who took small-pox.

DR. BURKE.—Had she a good *cicatrix*?

CHAIRMAN.—I believe so.

DR. DARBY.—I trust that my having offered some remarks on the last evening does not disentitle me from saying something on the present occasion. I think we are arguing this question altogether on a foregone conclusion; and I do not think, if you will excuse me for saying so, that we are taking a very general or a very philosophic view of the subject. At meetings such as this, when, considering the laws which influence the spread of disease, whether it be cholera, or scarlatina, or fever, or small-pox, we always try to get a too definite and distinct account of where the disease arose, and how it came into the country. I don't deny that there is such a thing as contagion. I believe there is, and that small-pox especially can be communicated by contagion. But—as I said on the last evening—I do believe that epidemic diseases, whether small-pox, fever, cholera, scarlatina, or other diseases of the kind, may and do all arise spontaneously without any possible contagion, infection or *materies morbi* being conveyed from one person to the other. I think that every man who has been as long in practice as I have must have seen cases where there was no possibility of tracing the cause of the disease to contagion. I do not believe that cholera is brought straight here by post, or by telegraph from Calcutta, any more than I believe that small-pox is, of necessity, brought here from England and elsewhere. I cannot believe that, because I have seen so much disease—small-pox as well as other forms—occurring in isolated parts of the country, arising as it were spontaneously and not spreading. Every man in the room knows that you will have isolated cases of cholera, fever, small-pox, and scarlatina which go no farther.

The first case of small-pox that ever I saw was in the Hardwicke Hospital—a black man, a sailor who was brought up from the quays. He was put into a ward where there were twenty other patients, and not a single one of them took it. There was no other case of small-pox in the ward at the time, and I never saw a worse case from that time to this than of that man who died of it in the ward. It never went further, and there was no epidemic of it at the time. When epidemics prevail there is some general influence—I don't know what it is—which spreads the disease from one individual to another; and any individual exposed to the direct influence of it may take it. It is said that small-pox is communicated through the clothes of persons going from one place to another, and thus spread all over the country; but what are we to say to doctors and clergymen who are always going about from sick-bed to sick-bed, and who yet do not bring it with them. I am sure that this meeting will not say that doctors, who see from 20 to 30 cases of that disease in an hospital, and who also attend private patients, ladies in their confinement, and children, are the cause of small-pox being spread about. I don't believe that I bring small-pox about with me. I don't think I do (laughter). Is it philosophic then—is it fair—is it rational, I ask you, to take that view of the subject? But the argument of all the gentlemen who have already spoken on the subject is, that the disease is spread by contagion alone. If that be really the fact, medical men and clergymen are nuisances, and ought to be scouted out of society. They ought to be put in quarantine (laughter). It is not rational, it is not logical, in my opinion, to argue the case on these grounds. What the epidemic influence is I honestly confess that I do not know. I do not know whether anybody else does or not; but I never heard anyone explain it in any way. At present, therefore, our knowledge is deficient in that respect. But I do not believe that we carry cholera, and typhus, and small-pox, and scarlatina about with us; and if doctors and clergymen, that go about from bed-side to bed-side, do not do so, I doubt very much that it is carried from one part of the world to another in other peoples' clothes. I ask these gentlemen who think so for their proof. I know there is great pains taken to trace where the disease comes from, but I think that trouble is useless and futile. When an epidemic disease occurs, it would be more philosophic, and likely to be more useful to try to ascertain if possible what is the state of the weather, the state of the barometer, the general condition of the atmosphere, which induces disease, rather than be trying to trace who brought it here. I dissent from the latter view of the case altogether. The experience of my whole life is against it. With regard to re-vaccination I have always been of opinion that it is a very bothering question. Small-pox is one of those diseases that is supposed to occur only once in a lifetime. There are many exceptions we know, and one remarkable instance was quoted here the other night of secondary small

pox. But as a general rule, small-pox is a disease that is supposed to come but once in a person's lifetime. Vaccination produces a disease to a certain extent identical with small-pox, and which takes the place of it; and I believe that if properly done it is protective for life. It is a disease that you have once in your life, and it is good for life if it is good for anything. In my opinion it strikes at the root of vaccination, to say that you are to go on vaccinating every five or seven years in an arbitrary manner, without anyone knowing the reason why, is mere dogma. The theory of small-pox being that it is a disease that occurs once in a lifetime, and vaccination being a small-pox, if you chose, if you take it once in your life it lasts for life. I believe that that is the case. I know, and I dare say every gentleman in the room knows, that a great deal of vaccination has been performed in an improper manner. A great number of medical men are most competent to vaccinate; but I will say that I have very often vaccinated a child, and never saw that child afterwards, and don't know whether it ran its proper course or not. I also know that there are a great many persons who used formerly to vaccinate, who did not know how to do so. I have known of medical men who did not know the proper course after disease. I know a medical man, who is now a public vaccinator, and who never likes to take the infection from any arm until the vesicle becomes opaque. He is a public vaccinator, and I believe his vaccinations are worthless. I knew another gentleman, not now alive, whom I prevented from taking—I won't call it lymph—but pus from an arm.

It was here intimated to the speaker that his time had expired.

CHAIRMAN.—Dr. M'Dowell, will you allow us to call on you. You have had great experience.

DR. M'DOWELL.—I feel greatly obliged to you, Sir, for speaking of me in this way; but I think what we want to-night is exact information, which I am not in a position to give. I knew that the subject of small-pox was under consideration, but I had no statistics at hand. We passed a very large number through the Hardwicke Hospital, reaching a total of 450; and of these I had one-fourth under my own care; but I can give no approximation as to the proportion of them that were vaccinated and unvaccinated. The mortality has been something about 17 or 18 per cent., and I know that it has been very much more amongst the unvaccinated, but I am unable to say how much more. But this I will say, that the impression on my mind very much corroborates what Dr. Grimshaw has mentioned. The very first case that came to the Hardwicke Hospital was an imported case. We had one solitary case of small-pox for several weeks in the Hardwicke Hospital; that of a young girl, a servant, who was landed sick from Wales. Very cruelly the family had

sent her away when she took the disease. Small-pox had been in the family, and she took it, and was sent across the channel. Upon arriving on the quay she was compelled by the police, who were on the look out, to come to the Hardwicke Hospital, and I can give day and date for its having been the first case of small-pox that we had had for a very long time in the Hardwicke Hospital. That case, I know, extended the disease to some others in the ward, and the focus thus formed may have extended the disease considerably. And I am quite sure that we could have counted the isolated cases—the imported cases—one by one, till at length they were to be counted only by dozens, and by fifteens, and now I am afraid, by many hundreds. I believe that it began in a very small way, and that means might have been taken to check the spread of the disease. One other point occurs to me as to the period of sending patients out from the hospital. Of course you may do a good deal by persuasion, but you can do nothing by compulsion. The patients are naturally anxious to leave the hospital. They see horrible sights, and a great many death-beds; and the moment they are able to walk they pray for their dismissal, and you cannot compel them to stay. You can only induce them by good diet to remain. You may wash them carefully, and use carbolic soap; but their clothes continue a focus of the disease. Many a man goes out with a clean skin, and his clothes infected with the disease; and the evil I believe is, that for every case that we cure, and send home, the disease is spread to five, six, eight, or ten cases. I often feel despondent when I think of the small amount of good we can do, and the amount of evil we are helpless to prevent. As to the question of re-vaccination, I would be very sorry to give any expression of opinion except this, that I am convinced of its advantage, if the reason were only this, that I am sure there are a number of people nominally vaccinated, but you can have no test as to whether they had been thoroughly vaccinated or not, until you test it by re-vaccination. In my own family, and amongst my own friends, I have met many persons who had been vaccinated and have marks; but who, on being re-vaccinated, have presented those typical vesicles; and I am convinced that they were to all intents and purposes unprotected, and I believe that a second vaccination diminishes the chance of having small-pox at last, and in a virulent form. I don't know whether it will be wandering from the subject to allude to the remarkable predominance of a purpuric complication with the present outbreak of small-pox. I believe that the extreme mortality—which is beyond the average I have no doubt—of the present epidemic arose from the large proportion of cases in which a purpuric complication was super-added; and I will say that I believe it is the remains of the epidemic that we recently had of *purpurea maligna*—acute purpuric disease. Since the occurrence, two years ago, of that disease which was so noted, I have seen isolated cases, one last summer, a typical case of purpuric disease,

in which death very nearly occurred, but which ended, however, very unexpectedly in a cure. Very recently we had two cases—one last week, in the Hardwicke Hospital—in which the premonitory symptoms were those of small-pox, severe rigor, vomiting, pain in the back, which are the typical symptoms of small-pox. There then came out a purpuric eruption, rapid and copious, and death occurred at the commencement of the fourth day, before there was time, as it were, for the development of the variolous eruption. I have no doubt that these were cases of small-pox in which the disease proved fatal, from the rapid development of the acute purpuric disease. I took the liberty of registering the last case as one of purpuric variola, though I could not detect any typical papules. There were, however, minute elevations of the cuticle, which I believe, if the patient had lived for twenty-four hours longer, would have been *variola* clearly developed. I have to apologize for having spoken unprepared. If I had been aware that I would be called on, I should have made some statistical notes as to the mortality among vaccinated and unvaccinated cases.

DR. ATTHILL.—I think some means ought to be taken to prevent the clothes of patients, who die of small-pox in the hospitals, from being given to their relatives. These clothes are either worn by their relatives, or are sold to be worn by strangers. The importance of this question has been strongly impressed on me by a case which came under my own observation. A poor woman, living in a lane close to my house, was sent to hospital, suffering from small-pox—she died; about a fortnight after her death I heard that the sister had died also. The latter had claimed her sister's clothes, had worn them, and the disease developed itself within ten days. There was no other mode of accounting for the infection. This is rather contrary to the theory propounded by my friend, Dr. Darby, as to the non-infectious nature of the disease. The clothes of patients, dying in hospitals, ought not to be given out, unless previously disinfected. The officers of health should take precaution to prevent this. With regard to re-vaccination, I may mention a fact recently stated to me by a naval surgeon. He was on board a ship which was engaged in the last Chinese expedition. Finding that small-pox was very rife on shore, he re-vaccinated every man on board his ship, and his ship alone, of all the vessels at that station—and there were several transorpts, as well as men-of-war there—remained free from the disease.

The Secretary then proposed an adjournment of the debate, in order that Dr. Stokes and other leading members of the profession, who had not yet given their opinions as to re-vaccination, might have an opportunity of doing so.

It was agreed that the discussion should stand adjourned to a subsequent evening, of which the members should receive notice.

Wednesday, February 14th, 1872.

DR. STOKES in the Chair.

The CHAIRMAN having called on Dr. Denham to resume the discussion on Dr. Burke's paper, that gentleman suggested that Dr. Lyons' paper, which dealt with the same subject, should be first read.

DR. LYONS said he had fully intended to have the paper ready that evening, taking for granted that at the rate of admissions of small-pox cases to the hospital, he should be able, against that evening, to complete the even number of 500 cases. It so happened, however, that there had been some lull in the admissions to public institutions, consequently his number was 20 short of that which he proposed to discuss; and as it would be more convenient in a statistical discussion to take an even number, he thought it better to defer his paper until the next meeting.

DR. DENHAM, in resuming the discussion, said that unfortunately the amount of information on the subject of re-vaccination was so very limited and vague, that he thought they were not in a position to speak with confidence on the matter. He had, however, dotted down two or three extracts from pamphlets and reports of cow-pock institutions, which had been written on the subject many years ago, and he would place them before the Society. The first was from a pamphlet written by Dr. Yeats, of Bedford, in 1803, and gave an account of a man who had cow-pock 53 years before the time he was inoculated with small-pox, and yet he entirely resisted the influence of the infection. The same paper mentioned the cases of other individuals, some 20, some 40 years of age, who had been under the influence of cow-pock, but were subsequently inoculated with small-pox lymph, and entirely resisted the infection. The reports of the Cow-pock Institution, London,* for the years 1800, 1801, and 1802, mentioned the cases of several persons who were marked with the small-pox, and who, when subsequently vaccinated, entirely resisted the influence of the vaccine virus. He wanted to establish not only that individuals who were brought fairly under the influence of vaccination would resist small-pox infection, but that those who were inoculated with small-pox were equally protected from the virus of cow-pock. It appeared also from these reports that of upwards of 50,000 persons who had been vaccinated, and who were subsequently inoculated with small-pox, not one well-attested case of the latter disease being produced could be found. All of those 50,000 persons, without any exception, resisted the influence of small-pox. At that time physicians went more philosophically to work than at present. They were not

* The medical staff of this institution consisted of 3 physicians, 3 surgeons, 3 consulting surgeons, and 3 visiting apothecaries.

satisfied with finding that vaccinated persons resisted the influence of small-pox generally, but they actually inoculated with small-pox 50,000 persons who had been previously vaccinated, and they always resisted it. He would also refer to a report of Dr. Rollo in 1804. He vaccinated 11,800 and upwards, and of these 2,500 were afterwards inoculated for small-pox, and in no one instance did the inoculation prove effectual. He might also observe that small-pox was not a preventive against its own poison, because there were many instances on record of individuals well marked with small-pox, who had taken the disease a second time and died of it. That was a very important point, showing that there were individuals peculiarly susceptible to the disease; and instances were known of persons being attacked, not only once, but twice or thrice. A most respectable practitioner, Dr. M'Veagh, of this city, mentioned to him the case of a friend of his who had seven different attacks of small-pox, and ultimately died of the disease. He would next bring under the notice of the Society a very extraordinary circumstance, communicated by Surgeon Dunning, being the result of experiments performed by himself in 1804. He was so impressed with the value of vaccination, and so anxious to establish the fact in the mind of the public, that he took a child of his, ten months' old, who had been vaccinated eight months previously, into a room where a patient lay in virulent small-pox, of which she died three days afterwards, and then and there inoculated it with some of the small-pox virus taken from the woman; but the child resisted it. The same man subsequently inoculated his two sons (one, twelve years old, had been vaccinated eleven years before). In order to make sure, he made three punctures on each arm of the boy, but the child successfully resisted the infection. The other child was three years and four months' old. It had been twice vaccinated and twice exposed to small-pox infection, being on one occasion brought into a room where there was a small-pox patient, and there re-inoculated, but on each occasion it resisted the disease. He (Surgeon Dunning) mentioned that that child was very slightly exposed to scarlatina—at least, a person having the disease lived within three doors of him. He had the child removed, but it was too late. The boy took the scarlatina and very nearly died of it. There were one or two other facts which he (Dr. Denham) wished to mention before he sat down. He had received a letter from a practitioner in Dublin, young Dr. Elliot, in which he stated that, within the last six months, he had seen two cases of small-pox after vaccination. He had also received a note from Mr. Shaw, of Talbot-street, who stated that he had vaccinated a woman three months ago, and she afterwards took small-pox. His impression was that vaccination, if once well established, could never be changed. He fully admitted that there were many individuals susceptible of the poison more than once. He did not stand up to condemn re-vaccination; but he thought

that, in many instances, it was unnecessary, and that thousands of those who were re-vaccinated would never have taken the small-pox, even had they not resorted to that means of protection. It has been calculated from some extensive statistics that five or six per cent. of those vaccinated are susceptible of the poison of small-pox. If re-vaccination would render those five or six per cent invulnerable to the disease, of course it would be desirable to re-vaccinate them, could they be discovered. But it must be admitted that the number of those requiring the protection was very small. I this day saw a man named Hugh Williams, who resides at 49, Nixon-street. He has had a very severe attack of small-pox. In this case his wife slept with him till the eruption was well out on him. His son, aged 13, slept in the room with him during the entire illness, and all the other members of the family remained in the house all the time, yet not one of them took the disease. Had they been all re-vaccinated, it would have been a powerful argument in favour of that practice.

In conclusion, permit me to say that while I have little or no faith in re-vaccination, I never refuse to perform it if parties are nervous on the subject and desire to have it done. But I do say I think there has been too much importance attached to it, and that when our hospital reports are fully before us we will find, perhaps, that nearly 5 per cent. of those who have been re-vaccinated have taken the small-pox. My belief is that one good genuine vaccination, as a general rule, protects from small-pox, not only for 7 years but for life ; while there are some few to whom it affords no protection whatever, and if those few could be selected and be re-vaccinated every year, they would still be susceptible of the small-pox virus, take it if exposed to the infection, and perhaps die of it.

DR. KIDD said the statement which Dr. Denham had made was too important to allow it to be passed over without some examination. He had referred to statistics which were collected very early in the history of vaccination. Vaccination was introduced somewhere about the end of the last century ; it was in 1796 that Jenner made his first successful vaccination of the human subject, and they now heard of a gentleman who, in 1803, stated that he had vaccinated 50,000 individuals, and that all these persons resisted small-pox. Now, he was sure Dr. Denham believed in the accuracy of the statistics he had adduced, but it required a greater stretch of good nature to believe in them than he (Dr. Kidd) was possessed of. He confessed he doubted the accuracy of the statement that in seven years after vaccination became known any gentleman could possibly have vaccinated 50,000 persons, and that these all resisted small-pox.

DR. DENHAM said that he had quoted from the report of the Cow-Pock Institution of London, and that the figures referred to involved the experience of a large number of persons engaged in vaccination.

DR. KIDD could not understand how the Cow-Pock Institution could at that time have well ascertained facts to justify the statement. Then there was the statement of a gentleman who said he had vaccinated 11,800 persons, and of these 2,500 resisted the influence of inoculation. That required a little consideration also. The question was, what were they to consider a success in inoculation for small-pox? Must it be a vesicle that would run every stage of the disease; or was the production of a vesicle in a shorter period a proof of it? He put in connexion with that this fact, which rested on unquestionable authority, that before vaccination was introduced, when there were professional inoculators who went through England inoculating people for small-pox, and when a mild virus that produced only one or two pustules was esteemed of immense value, the professional inoculators kept up the supply of virus on their own arms. He had himself vaccinated a gentleman, his own brother-in-law, who, to his own knowledge, had had small-pox, and he never saw a more perfect or beautiful vesicle than was produced in that case. The gentleman had small-pox 18 or 19 years ago; and about a year and a-half ago, when small-pox was prevalent in London, he had occasion to go there and was vaccinated with the result stated—the production of a perfect vaccine vesicle.

THE PRESIDENT.—There are two questions—first, the power of vaccine in preventing small-pox; and next, the power of variola in preventing vaccination. You are surprised that variola does not prevent vaccination.

DR. KIDD said he was not surprised at it. On the contrary, he asserted that the vaccine disease once established, will not prevent the formation of a perfect vesicle on re-vaccination. That the previous occurrence of small-pox will not prevent the formation of a vaccine vesicle on re-vaccination; and further, that the previous occurrence of small-pox by inoculation does not prevent the formation of perfect vesicles on re-inoculation. It is true, there are certain persons who seem to be insusceptible of the vaccine disease, both primary and secondary, but their number is very small. He had re-vaccinated a large number of persons, and with the exception of three or four, he had not met with any who would not take the disease. In the Prussian army it was found that some 47 per cent. failed to take the disease on being re-vaccinated; but in the Prussian army they were satisfied with one trial, and believed that those who did not take it were incapable of doing so. But in the Danish army they gave those who failed in the first instance another trial, and they thus found but 17 per cent. to resist the influence of re-vaccination. He believed the fact to be, that where re-vaccination did not take it was because the virus had failed to get into the system, with the exception of the few individuals referred to who were incapable of

the disease. The difference between re-vaccination and primary vaccination was this, that re-vaccination would run its course much more rapidly than primary vaccination. Early in the history of vaccination, Dr. Bryce proposed a test as to whether the vaccine disease had affected the constitution of the individual. On the fifth or sixth day after primary vaccination he re-vaccinated the child, and he found that the second vesicle ran so rapid a course that it overtook the first, and arrived at maturity at the same time. Undoubtedly it ran this rapid course in consequence of the primary vaccination. He believed the same thing held good throughout life, and that if a person were vaccinated once, and if he were still under the influence of the first vaccination, the second would run a more rapid course. Sometimes it was said it failed, but it had not failed; in fact, it would form a perfect vesicle, and show in a very short time the true course of the disease. The same thing applied to small-pox. It was observed by Dr. John Thompson, long ago, that the difference between primary small-pox and secondary or modified small-pox was, that the modified small-pox ran its course more rapidly than the primary small-pox. He denied, then, that vaccine disease once established prevented the recurrence of vaccine disease; it merely caused the secondary vaccine disease to run a rapid course. Dr. Denham stated that 6 per cent. of those vaccinated would take small-pox.

DR. DENHAM.—I took the statement from you.

DR. KIDD said he had stated at the Surgical Society that there were statistics to prove that from six to twelve per cent. of those vaccinated would, on being exposed to small-pox, take that disease. Although he entered fully into the subject at the Surgical Society, perhaps he would be permitted briefly to recur to it. He made the statement on these facts. About 1828 there was an epidemic of small-pox in Marseilles. The Academy of Medicine in Paris made a census of the town, and ascertained the number of those living in the town who had been vaccinated, the number who had been inoculated, the number who were utterly unprotected, and the number who had taken the small-pox in each of these conditions; and they found that six per cent. (6·6) of those who were vaccinated took variola. In Newark-on-Trent, in 1839, there was a severe epidemic of small-pox, and there Dr. Bigsby made a survey of the east ward of the town, and he found that about the same proportion (5·01), as in Marseilles, of those who had been vaccinated took the disease. It was upon these two statements that he said he believed six per cent. of the persons who had been vaccinated would take small-pox on being exposed to the infection. There was an instance of a smaller community exposed to a still more concentrated form of the infection—that was in one of her Majesty's ships at sea, the "*Octavia*," on board of which there were 610 officers and men, and it was found that more than ten per cent.

of those in the ship who had been vaccinated took small-pox, whilst all those who were unvaccinated—21 in number—were attacked by small-pox. He therefore ventured to say that under ordinary exposure six per cent. of the vaccinated would take the disease, and ten to twelve per cent. under concentrated exposure, such as in the case of the ship referred to. They had thus the fact established, that a certain number of the vaccinated would take small-pox, and they had instances where small-pox had broken out in communities, and had ceased when the individuals were re-vaccinated. They had the remarkable case adduced by Dr. Moore, which occurred recently in Glasgow, and the case described by Dr. Marston, where small-pox broke out in a deaf and dumb institution, and immediately ceased on all the children being re-vaccinated. These two facts were, he thought, sufficient to warrant them in recommending re-vaccination, viz., that from six to twelve per cent. of those who were vaccinated would take small-pox on exposure to it, and that the disease, when it broke out in a community, ceased when the individuals composing that community were re-vaccinated.

DR. HENRY KENNEDY said that Jenner's first book appeared in 1793, and all knew that it at once gained the greatest popularity, and it was only after several years that any serious opposition was offered to its views. So that Dr. Denham's statement, that 50,000 had been vaccinated by the year 1804, was entirely within the range of credibility. Dr. Kennedy then went on to say he wished he could look upon the question in the same light as Drs. Kidd and Denham, who had just spoken. He recollected the time when a death in a person who had had a good mark on the arm was exceedingly rare. But within the last year his confidence had been rudely shaken; for he had both seen and known of several instances where death occurred with often good vaccination, and re-vaccination; and the returns in both London and Dublin fully bore out this statement. Except, then, as a great modifier of the disease, he could not look upon vaccination as a complete prophylactic; and his views of former years had to be so far altered. The explanation appeared to be that the small-pox was now an epidemic, and of a very severe character; and against it vaccination seems to have lost some of its power. As showing the epidemic character of the disease, he might allude to a point spoken of by Dr. Grimshaw on a former evening, viz., that cases of fever had occurred, and he had seen several himself, where all the incipient symptoms were those of small-pox; and yet the cases did not turn out to be such. These cases reminded him how the epidemic influence tinges, as it were, other diseases—a point noticed by the older writers. Something analogous to this occurred a few years back, when spinal arachnitis prevailed; for symptoms of this affection often showed themselves in the very middle of both typhus and typhoid fevers.

DR. LAW said he happened to be connected with a nurses' training institution in Holles-street. Twelve nurses were constantly employed. Eleven of them were re-vaccinated, and one was not. The eleven, although in constant attendance on small-pox patients, never contracted the disease, whilst the twelfth, who was not re-vaccinated, got it. At that moment he was attending small-pox patients in Sir Patrick Dun's Hospital, and they had received into hospital since the 1st of January 50 cases—15 females and 35 men, and there was only one young person among them, that person being ten years old, none of the others being younger than fourteen. This was an interesting fact, as they knew that children were more susceptible of that class of disease than adults; and therefore, unless they could suppose the vaccine matter afforded them protection for a time at least, more children would have been recorded suffering from small-pox. The fact was recognized in the Prussian army, where re-vaccination was performed every seven years, because it was thought the vaccine matter lost its protective influence after that time. He thought the fact referred to by Dr. Denham, of persons who were vaccinated and then re-vaccinated and resisted it, showed that the vaccine matter had not lost its effect. They should have watched some time longer before performing re-vaccination. In every case he had had after vaccination the disease had been modified, and if he were certain that small-pox would come on after re-vaccination he would still urge its performance. In no case did the secondary fever occur, and even the worst cases were over on the 9th or 11th day. He ventured to hazard the opinion that this secondary fever which occurred in small-pox was really pyemia, due to the absorption of purulent matter into the system.

DR. BURKE, in reply, said his paper was a tentative one. It was merely drafted out by him with a view of gaining information from his brethren, and such information as would be beneficial to all. He was sorry that so few of our Hospital Physicians had come forward on this occasion and given their experience and their statistics as to the number of cases treated in their hospitals, and with what result; and he was glad to find that his friend, Dr. Lyons, would on a favourable opportunity favour the society with his experiences in the Hardwicke Hospital. He (Dr. Burke) had stated that he had nothing new to communicate. He merely wished to lay before the society such information as he in his capacity of a public servant had been put in possession of, and it was their business to draw deductions from the facts stated. A great deal of matter had been introduced into the discussion about vaccination and re-vaccination. Statistics had been brought forward, but in many cases the basis on which these statistics were framed had not been stated. He recollected, many years ago, one whom they all knew and respected, having a controversy

with Sir James Simpson, of Edinburgh, as to the treatment of placenta previa. Sir James Simpson wrote to Dr. Johnston and said—"My statistics prove to demonstration that I am right." Dr. Johnston's answer was—"My dear Simpson, my opinion of statistics is analogous to that of Sam Weller on sausages—"They're werry good, purvided you knows the young woman as made 'em.'" (Laughter). Now his (Dr. Burke's) impression was that once vaccination was properly done, the individual would not be better by re-vaccination; but, as so many had said, if re-vaccination gave the patient confidence, and therefore did him good, of course the physician would perform it. Dr. Beatty read his recantation there; but at the Surgical Society he was against re-vaccination. He spoke pithily, and said:—"Supposing you are requested to re-vaccinate, and refuse, and your patient gets small-pox, it will never be forgiven you." As to getting small-pox after vaccination, he forgot that some persons had been attacked by small-pox three times, and there had been deaths during the present epidemic of some who had previously had small-pox. They knew that measles, whooping-cough, and scarlatina, were diseases that generally occurred once only in life; yet he supposed the youngest practitioner there had seen persons attacked with measles, scarlatina, and whooping-cough a second time. Nay, he had a return from the district of Keady, of an old lady who had died from whooping-cough at the age of seventy-six. If such be the case, was it surprising that people should catch small-pox after being vaccinated? There was another observation which he wished to make, paradoxical as it might appear, but it has not escaped the keen sense of his friend Dr. Maunsell, viz.—that the carrying out of the Vaccination Act had been the cause of the great extension of small-pox in this town. So many got small-pox in such a modified form that, to use the words given by Dr. Maunsell, "They put it over them on their feet." Children were going about in lanes and alleys, having small-pox upon them, disseminating disease; and that was the result of the successful carrying out of the Vaccination Act in Dublin.

Dr. Foot read the following paper:—

On the Treatment of Small-pox. By ARTHUR WYNNE FOOT, M.D., T.C.D., Physician to the Meath Hospital; Fellow of the King and Queen's College of Physicians in Ireland; Diplomat in State Medicine of Trinity College, Dublin.

It is with very great hesitation that I venture to occupy even a portion of the time of this learned Society with any remarks upon the treatment of small-pox, because there are present gentlemen whose opportunities of observation upon this disease are vast compared with mine, and whose experience of variola is not of yesterday, as mine, comparatively speaking, is.

The total number of cases of small-pox which I have treated during the present epidemic amounts to but 59, but each of them I have personally watched most closely, assisted in my cases in the Meath Hospital by a number of well trained and highly educated clinical clerks. In 57 of those cases thermographic charts have been carefully kept day and night; quantitative analyses of the urine have been frequently made in the laboratory of the hospital; the bodies of the dead have been opened whenever it was possible to do so; of each individual case I have kept the notes myself, independently of others; and no expense or trouble has been spared to procure apparatus or medicine of the most modern description. I mention these facts merely for the purpose of endeavouring to invest the conclusions derived from this small phalanx of cases with some fractional part of the authority which appertains to those derived from enormous hosts of cases.

The principle upon which I treated these cases has been that known as the antiseptic method, and this principle was carried out because I believe it to be the plan of treatment best adapted to save life, to prevent deformity, and to check the propagation of the disease. Surrounding the central principle of the antiseptic method are multitudinous details which cannot but suggest themselves to a careful and an anxious physician, and without attention to these minutiae the routine administration of antiseptic medicines may often prove very unsatisfactory.

My remarks on the treatment of small-pox shall have special reference to the severest forms of the disease, and to those coherent or semi-confluent varieties which come nearest in violence to the fully developed malady. I shall not delay over the management of the mild, the uncomplicated, the modified types, which will get well under any mode of treatment, or perhaps most quickly if left completely alone. Not that I would be careless about mild cases of variola, but because it is most important that if any decided principle of treatment is to be submitted to the crucial test of experience, the experiment must be made on cases which have *not* a tendency to get well of themselves. In decidedly mild cases of variola in a sound constitution the tendency to recover is obvious; in decidedly confluent cases, no matter what the constitution of the patient may be, the tendency to death is obvious—therefore, any method of treatment which may be at all *sub judice* as to its efficacy must be applied to the latter class of cases, and to those alone, if its value is to be properly tested.

Of confluent cases I have had 24; of semi-confluent, 6; and out of these 30 cases 11 have died—a mortality of more than one-third. This does not look well for any particular plan of treatment; but I have not much apprehension that I shall be thought singular if I rank confluent small-pox high in the list of the *morbi majores*. I believe variola vera to be a disease as dangerous to life as is Asiatic cholera, and that its usual

rate of mortality is 50 per cent., unless the constitution of the epidemic be an unusually mild one, such as I think there is no reason to consider the present visitation to be. It is also to be borne in mind that nearly all these confluent cases were presented to me under the most unfavourable circumstances—brought late to hospital, badly fed beforehand, intemperate, one of them far advanced in pregnancy, three of them never vaccinated (and no better evidence of this operation in most of the others than a family tradition), two others, children under nine years of age, one of whom had been previously exhausted by chronic scrofula in the bones. The fact that some of these fatal cases made any resistance at all to the disease seemed to me a proof of the value of the treatment adopted.

The way in which I endeavoured to carry out the antiseptic treatment was by giving carbolic acid internally in the shape of the sulpho-carbolate of sodium, and when more suitable the sulpho-carbolate of iron, giving the sulphurous acid of the Pharmacopœia, diluted with water, as the usual drink, using gargles of sulphurous acid, spraying the larynx with it, washing the nares and upper surface of the soft palate with solutions of sulphurous or of carbolic acid, keeping carbolic oil to the face, washing the body with solutions of sulphurous acid or vinegar and water, throwing pure sulphurous acid about the bed and bed-clothes of the patient, and burning sulphur in the room, so that the sick might breathe for some portion at least of the day an atmosphere charged with some sulphurous acid gas in it.

After much consideration of the subject I have adopted the opinion that the secondary fever of confluent and semi-confluent cases is due to the presence in the body of products of decomposition, which commence to be formed as soon as the lymphic contents of the hitherto vesicles become purulent, rather than that it results from the dermatitis which springs into existence at that period, and which I consider to be the necessary consequence of the irritation of the now numerous sub-epidermic abscesses; and believing that in carbolic acid, used both externally and internally, there is an agent capable, when it can get fair play, of checking the decomposition of the pus, or of paralysing the effects of the products of its decomposition, I considered the first thing was to ascertain the best mode of its administration.

The acid may be given internally in either of three ways—the plain acid in solution, or as a carbolate, or in the form of the sulpho-carbolates introduced into medicinal use by Mr. Sansom. In France Chauffard* has used the plain acid in the worst cases of variola, selecting those most likely to prove fatal, and he considers it to have a specific action on the secondary fever.

* *La Revue Médicale*, April 16, 1870. *Pract.*, Vol. v., p. 54.

The direct compounds of carbolic acid—the carbolates—are unsuited for medicinal use, because they are unstable, unpleasant, and for the most part insoluble—the pure carbolic acid is better than them, but it has disadvantages which the sulpho-carbolates are free from, and my choice between these three ways of using carbolic acid internally fell upon the sulpho-carbolates, because they are stable compounds, soluble, and not peculiarly disagreeable. Of xylol or dimethyl benzol I have had no experience.

I have given the sulpho-carbolate of sodium in 34 cases of small-pox, in doses of from 7 grs. occasionally, to 60 grs. every third hour; it is very soluble, and can be taken in plain water, or if its earthy-saline taste must be disguised, it can be given with some infusion of orange-peel or of cascarilla. During its administration carbolic acid is eliminated by the lungs, its odour being very perceptible in the breath, and the sulphuric acid and soda pass off by the kidneys. I have not observed it to cause any sickness of the stomach or unpleasant feeling in the head, even in very large doses; children have no objection to it. I have also verified the observations of Mr. Sansom, that subsequent to its administration the factor of the evacuations from the bowels is greatly lessened, the urine is unusually slow to decompose, and the flesh resists putrefaction. I had opportunities of remarking the latter fact in making *post-mortem* examinations. I never found any one to complain of the usually nauseous variolous odour of the skin.

At the same time, in all confluent and semi-confluent cases I use sulphurous acid in every form and way in which it can be applied. There is no hostility between these two agents—carbolic acid and sulphurous acid—the power of both combined is very great. Their simultaneous use may be compared to the driving of a plough with a bullock and a horse; the agents may be different in appearance, but their action and work is to the same end.

One of the simplest and most effective ways of exhibiting sulphurous acid is in the gaseous form; flowers of sulphur dropped on a heated shovel and carried about the room with its pale blue flame, forms by its combustion sulphurous acid gas, which, diluted with the nitrogen of the atmosphere, can pass into the lungs of the patients; and I consider that there is reason to believe that this practice three or four times a day is beneficial to the attendants and other inhabitants of the house as a prophylactic. Irritation of the bronchial membrane soon gives notice when there is as much sulphurous acid gas in the atmosphere of the room as is consistent with health; it specially and soonest affects any one with bronchitis, and I should say it ought to be used very carefully if the variolous patient were labouring under that affection. One of the nurses at the hospital who suffered from chronic bronchitis used to be greatly affected by the daily fumigations

if in the rooms when it was being done, and used to feel its effects long before and long after any of the patients or other attendants. Some persons in health also have by idiosyncrasy a condition of the lining membrane of the air passages which makes them peculiarly sensitive to this gas, even in a very much diluted form, and in such a reflex cough is very quickly excited. It is not to be supposed that the principal object in burning sulphur is to disinfect the room; this is an after-consideration, and would require an amount of sulphurous acid gas dangerous if not fatal to life; but it is to develop as much of this antiseptic agent as may be safely inspired, with the view of checking the multiplication of the small-pox poison in the person of the patient, very minute quantities of sulphurous acid being capable of arresting fermentation.

The sulphurous acid of the Pharmacopœia undiluted I frequently apply in an atomized vapour to the nares and pharynx, through vulcanite tubes, curved or straight as may be required, in the manner recommended by Dr. Dewar of Fifeshire. I have found that patients like it, and eagerly ask to have the operation repeated. It removes disagreeable tastes from the mouth, keeps the nose free from obstruction by accumulated crusts, and much of it must reach the lungs. A few whiffs open the nose when it is stuffed, or when, as hospital patients say, the head is stopped; the spray has not the suffocating odour of the bottled acid.

I give them the acid internally several times in the day, or for a drink both day and night when there is much thirst, in drachm or two drachm doses at a time, diluted with water; one drachm of the acid in two wine-glasses full of iced water is a very pleasant drink; less water may be used, a drachm may be taken in a wine-glassful of water, but if the acid has been freshly prepared, or very well kept, the drinking such a solution may catch the breath. If the person drinking the acid in the strength of one drachm to two ounces of water will avoid inspiring through the nose when the glass is brought to the lips, and swallow the liquid in gulps, the vapour cannot irritate the air passages. I always have the acid added to the water which the confluent and semi-confluent cases drink, and they like its acidity very much. I frequently take it myself as an agreeable and wholesome beverage, and advise the students to do so, as a prophylactic. I prefer the sulphurous acid to the sulphites or bisulphites, because they are so unstable in solution—the form in which they must be given—rapidly absorbing oxygen, and passing to the state of sulphates, neither have they much to recommend them in preference to sulphurous acid on the score of cheapness, purity, or flavour.

As confluent variola is fatal to life, principally through its complications, it is these latter which call for the most immediate attention. Among the principal complications, arranged in the order of frequency

in which I have met them, are laryngitis, delirium, hæmorrhage, albuminuria. Laryngitis in some degree is an almost inevitable event in confluent variola, and is, perhaps, usually the most frequent immediate cause of death. I have endeavoured in all severe cases of variola to anticipate this event; this I think is the way to combat it; but if the case is not got in time, of course this plan is out of the question. When I have had an opportunity of treating a case from the commencement, or from an early period, upon the very first complaint of any kind about the throat, and sometimes the answer to the constant inquiry will be only a "queer feeling" in the neck, I surround the neck with cotton-wool, spray the larynx and pharynx with a saturated solution of tannic acid, give ice and mop the throat with glycerine of tannin. I afterwards, when necessary, apply leeches freely to the neighbourhood of the thyroid cartilage, followed by poultices and hot sponges. I also use sprays of solution of carbolic acid, 5 m. to the oz., solution of sulphite of soda, 60 grs. to the oz., or the sulphurous acid B. P. undiluted.

The tannin and the carbolic acid are calculated to check the formation of the eruption which produces the irritation and the œdema of the parts; these sprays are applied by a curved tube, which while the tongue is held out, can be carried quite into the pharynx, and pointed straight downwards, so as to gush the spray into the larynx, producing cough and expectoration of tenacious mucous, which is attended with relief. If these local remedies are not used early, the condition of the mouth and palate will very much interfere with their employment, so when the lips and cheeks and tongue are too much swollen to admit of the mouth being opened sufficiently to apply the spray *per oram*, the nostrils can be drenched by the spray, conveyed by a straight tube, and the head being held well back, the solution trickles down to the lower parts of the pharynx. In this latter way also the very great annoyance caused by eruption on the upper or posterior part of the soft palate can be relieved. If these external and internal local applications supported by the general antiseptic treatment and active purgation with castor oil fail, after having been carefully persevered in, and stridor, dysphagia, and dyspnœa continue to increase, I do not know what remains to be done; I have never contemplated any surgical interference with the wind-pipe, but I am far from thinking that there are not certain cases of sudden œdema in which life could be saved by an operation, and by no other means. Of any single remedy ice has certainly given the most relief in laryngeal complications, and has been in almost constant use night and day, in the small-pox wards; the swallowing of the melted ice also relieves the salivation and dysphagia resulting from the irritated mouth and pharynx, and next to ice the patients seemed to like, from its immediate, though often only temporary relief, the spray of carbolic acid, especially when sent through the nose. Sometimes I thought that the coagulating powers of the solution of carbolic acid hardened the

musus which flows profusely from the Schneiderian membrane, making plugs in the cavities of the nose, and then I washed out the passages with plain water. Anything which favours the removal of the accumulated crusts gives great relief, and the injection of a stream of cold water often seems, by a contraction of the mucous membrane, to set free the plugs, and lead to their expulsion. When the exanthematous process affects the Schneiderian membrane extensively, and it appeared to me to do so most in confluent cases in children, because in them I observed the greatest amount of nasal discharge, and the greatest tendency to epistaxis, the spraying of the cavities of the nose is very important, particularly as Portal^a has found the sphenoid bone carious in several children who had died of small-pox. The variolous irritation may, however, sometimes produce a beneficial change in the secretions of the nose, as in one of my cases, a man aged 22, who contracted semi-confluent variola, while under treatment for ozæna, the ozæna had quite disappeared when he recovered from the small-pox; the variolous odour from his skin was very offensive, and lasted for an unusually long time; this particular case was not treated on the antiseptic method.

In connexion with delirium I may allude to the treatment of the headache and sleeplessness of severe small-pox. The cases presenting delirium which I have met with have almost always been in previously intemperate persons, of whom, either from themselves or through their friends, it was ascertained that they were habitual drunkards, getting drunk systematically once a week or oftener. I have had fatal cases of the most confluent kind without any delirium at any period in persons of temperate habits. In two cases of confluent small-pox in total abstainers, there was a remarkable freedom from this symptom at any period of the case, and I consider that one of the principal agents in inducing delirium is previous habits of intemperance. In a grocer's assistant, a brewer's drayman, and a coal heaver employed in lighters on the river, whose propensities to drink were fully certified to, this delirium was very troublesome, not that they were dangerous to themselves or the other patients, for I noticed no suicidal tendency, nor were any of them furious, but they could not be kept in bed unless some one was constantly by them; they were perpetually rambling about the wards and into the female apartments—one got as far as the avenue gate. They were obedient, and easily persuaded to go back to their beds, but as soon as the attendant's back was turned, out they would get again. This required frequently two night nurses—male and female—and sometimes a third, when more than one such patient happened to be in hospital at the same time, and the getting these patients to stay in their beds was like trying to keep a set of lively leeches to remain in an open crock—no sooner

^a Anat. Med. Tom. 1, p. 138.

was one put back than another was out. The hands and arms trembled, and there was great loquacity with the restlessness, so that in many points their condition bore a strong resemblance to delirium tremens. I never allowed the straight waistcoat or any mechanical contrivance for restraint to be employed, and have gone at unexpected hours of the night to see that my orders in this respect were strictly obeyed. With the imtemperate I found stimulants do more good than anything else in this locomotive delirium; they succeeded when lupuline, bromide of potassium, and chloral were of no use; opium did not answer at all. As each case of delirium must be treated on its own merits, no general rule can be given for the administration of remedies for this complication. In the case of children I think any kind of stimulant generally did positive harm, and leeches behind the ears, with cold to the head did most good. For the headache and weight in the head, leeches were certainly very beneficial to all, particularly when indicated by epistaxis or congestion of the conjunctivæ. I was surprised that the leeches should so readily bite the variolous skin, even when the eruption was confluent and mature, and carbolic oil had been applied; the nurses told me they always prepared the part for them by rubbing it well with sugar and milk, and nothing else, and that the leeches would then bite at once. The restlessness and insomnia when there was no headache or disturbance of the sensorium, was treated in adults with black drop or Battley, which also usually made the skin act—a very desirable thing where its secreting function is so much embarrassed; to children, of whom I had 7 under 10 years of age, I gave chloral to make them sleep when they required it, which was not often. The hair of all patients was cut close whenever there was much disturbance of the head; this helps to save it from ultimately falling out, as it is very apt to do after confluent variola, prevents much trouble in keeping it clean, and facilitates the employment of cold applications.

With reference to hæmorrhage as a complication, I had but one case of true hæmorrhagic variola; no papular eruption appeared, the upper half of the body was of an equable and deep-tinted scarlatina-form redness, while the lower extremities were covered with petechiæ and maculæ. Death, which took place on the fifth day, was preceded for some time by profuse menorrhagia and hæmaturia; muriated tincture of iron was given during the short time the patient was in hospital. The hæmorrhagic variety of variola was very frequently observed in the recent Parisian epidemic, and M. Briquet, in a communication on the subject to the Académie de Médecine, attributed its prevalence among the soldiers to their bad diet and the hardships they had to undergo during the siege. In the Hampstead Hospital over 400 cases of this form have come under observation in ten months, and the medical superintendent, Dr. Robt. Grieve, has observed, in reference to those cases, that

vaccination does not prove a preventive, the majority of cases having had some kind of marks; that certain occupations seem to predispose to this type of the disease, such as necessitate much exposure to heat, as, in males, engine-drivers and stokers; in females, cooks and kitchen-maids suffered from it; that it is very rare in children; and that the respiration is a better guide to prognosis in this form than is the pulse or temperature, an increase in the number of respirations being most unfavourable.* More than half the adult females whom I have had charge of menstruated early in the disease; with none of them was it the due time for this event—one had not menstruated for three months previously; one, who had tuberculous deposit in each lung, and in whom the catamenia had become scanty and irregular, was pleased to find this function become re-established. When the discharge from the uterus threatened to become profuse and exhaustive, I had a block of ice rolled in a towel kept against the vulva, and gave 15 grain doses of gallic acid, with 30 gtt. of liquid extract of ergot of rye, every two hours till the hæmorrhage was checked. When not excessive I did not interfere, as it seemed to give much relief to the lumbar pain, and also to headache. It usually occurred during the first week of the disease.

A peculiar appearance which I have several times observed in the urine during the administration of large doses of the sulpho-carbolate of sodium may cause alarm. It is a smoky tint, as if the colouring matter of blood were mixed with it, and in bad confluent cases, in which hæmaturia may be at any moment expected, this appearance will justly cause uneasiness. The spectroscope at once shows that the appearance is not due to blood-pigment, and there is little doubt but that it is due to the presence in the urine of carbohc acid, or of some kindred substance, probably some of the various compounds formed from it. It is well known that carbohc acid applied externally, as to the surface of a stump, has caused melanuria, so also has the external use of tar, the internal use of creasote is known to produce this phenomenon too, and creasote itself has been procured from the urine of persons taking it, by distillation. I regard this discolouration as a strong proof that in persons taking sulpho-carbolates, carbohc acid or some allied substance passes through the kidneys. The spectroscope, the microscope, and the absence of albumen will show that the colour is not due to blood.

In connexion with this matter I may observe that I think albuminuria in confluent small-pox a symptom of the gravest importance as to prognosis. In 10 confluent cases I examined the urine for it at the height of the disease, and found it in but 3—those 3 died. None of them had hæmaturia, nor was there any history of previous renal disease. Quinquaud says its presence is frequent in alcoholic patients, rare

* B. M. J., 21st Oct., 1871, p. 465.

when variola occurs in healthy subjects. In a communication made to the Medical Society of Lyons,^a September, 1871, on this subject, M. Cartaz says albuminuria is met with in confluent variola in about the proportion of 1 in 5 cases. It must of necessity be present where there is hæmaturia, or previous Bright's disease, and its presence, when unaccompanied with blood, would certainly indicate warm baths.

In the secondary fever quinine is of great value, both on account of the anti-pyretic action by which it reduces the temperature, and also because it is an antiseptic itself of very high rank, and so peculiarly appropriate in a fever connected—to use the very mildest term—with the process of decomposition going on so extensively over the surface of the body. Binz's observations on the destructive power of quinine upon the organisms, which, at all events if not the cause, have much to do with septicæmia, and Helmholtz's enlightened application of those observations must be known to all.^b Quinine, and, when the temperature has been reduced, the sulpho-carbolate of iron in from 5 to 15 gr. doses three times a day, or oftener, help to restore the shattered strength, and to check the tendency to secondary abscesses or consecutive pustular eruptions.

The treatment of the face, I think, lies far deeper than that of the surface—it lies in lessening the suppurative process by an abortive treatment applied from within by means of antiseptics. I certainly regard carbolic and sulphurous acids as having the power of lessening, and of more or less actually preventing the conversion into pus of the contents of the vesicles, but they must be used early, as soon as the multitude of granular papules announce a confluent case. I assist them by the external use of carbolic oil kept on the face from the very first appearance of the eruption. Coarse skins bear a strength of one part of the acid to three of oil, but thin skins are smarted by this proportion, and one part to seven is most comfortable for them. If the smell be objected to, half a drachm of the oil of origanum to 8 oz. of the carbolic oil will completely hide it. The only other external application which I would employ is flexible collodion, but it must be applied to the papules as early as possible, and it cannot be used to all in the most confluent cases. It must be the flexible collodion, for any other is endosmotic to air from want of a colloid ingredient. The late application of any collodion only does mischief—it forces the pus to burrow backwards into the cutis, instead of coming forward and elevating the epidermis, thereby increasing the dermatitis and ensuring pitting, which depends much more on the depth of the pustules than on their number or their size.

In concluding these remarks, which have of necessity been of a disjointed and desultory character, omitting many points that time would

^a Med. J. & Gaz. 16th Sept., 1871, p. 35-6.

^b Pract., Nov., 1869, p. 304.

not allow to be touched on, I would observe that I do not bring forward the use of the sulpho-carbolates as anything new. I learned to use them in other diseases before this epidemic appeared, from seeing their efficacy in the hands of our distinguished President, Dr. Hudson, and to Mr. Sansom is due the credit of their introduction into medicine. Neither do I pretend to think of them as in any way specifics, for I think they are hardly able even to influence the course of bad confluent small-pox, if it has been allowed to reach its height before they are employed. When the house is on fire from top to bottom, the fire-engine, which would have been of service earlier, is then almost valueless. Nor can they avail against mechanical obstructions to life, such as the closure of the glottis, or the opening of blood-vessels. Too much is not to be expected of them or claimed for them; they are but one of the various means of introducing antiseptic substances into the body, and may be, before long, superseded by other substances, which may be able to do a greater amount of work, and in a better manner. I have almost always used sulphurous acid along with them, because I have not been making experiments to test the action of the sulpho-carbolates as new medicines, but endeavouring to cure my patients, and with that object I selected these salts as happy productions of the modern alliance between the science and the art of medicine.

The principle of the treatment which I have endeavoured to carry out lies in the taking advantage of the known hostility between these two powerful antiseptics, carbolic and sulphurous acid, and all fermentative processes, especially that of decomposition. My results may not appear to be very favourable, but, considering the circumstances, they are such as to forbid me to regret having adopted the antiseptic method of treatment, and to induce me to persevere in carrying it out until I shall have found a better one.

The CHAIRMAN said, they were deeply indebted to Dr. Foot for his able and interesting paper. He thought there was nothing more difficult for a physician than to establish the value of any medicine in a disease that came under the law of periodicity; but as far as they could see, Dr. Foot had made out a very important case in favour of the properties of carbolic acid in cases of small-pox. He had been in the habit of believing that the analogies on which the carbolic treatment was founded were somewhat loose; but he must say, from what Dr. Foot had said, and what he had seen himself in the wards of small-pox, his opinion was greatly modified. He had himself used the sulpho-carbolates, and had been surprised at the great rapidity of the convalescence, and the great rapidity of the disappearance of every trace of the disease from the skin.

The Society then adjourned.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

Saturday, January 20th, 1872.

DR. KIDD, President, in the Chair.

The Secretary read the following paper :—

Case of Premature Quadruple Birth. By WM. CUPPAIDGE, M.B., Castlereagh.

THE occurrence of quadruple birth in the human subject is of extreme rarity. The records of the Rotunda Lying-in Hospital, though covering a period of over one hundred years, and including nearly two hundred thousand deliveries, only show one example of a pregnancy of four children. A case of this kind having lately come under my hands, I deem it a duty to lay the particulars of it before the Obstetrical Society. On the 25th October, I was sent for, at 11 a.m., to see a patient who was attended by a midwife, and reported to be in active labour. She was nineteen years of age, it was her first pregnancy, and she had been married on the 26th of last April, so that she was just about entering the sixth month of utero-gestation. On seeing her, I was at once struck by the great size of the abdomen, which was fully as large as it usually is at the full term of pregnancy. On making an internal examination, I found the os uteri open, about the size of a shilling, with the membranes protruding. I left her in charge of the nurse, and returned at 5 p.m., and, just as I got into her room, a small female fœtus, of about the fifth month, was expelled, head foremost. The woman's size not having diminished, I examined and found another bag of membranes pressing through the os. Supposing that this second fœtus would be expelled as easily as the first, I went away, leaving directions with the nurse to send for me if she saw any necessity. There was no return of pains, and the patient slept during the night. When I visited her, at 11 o'clock the next morning, there was no pain present, and matters were just as when I left her last evening. I therefore gave a dose of ergot, which was soon followed by the expulsion of a *second* fœtus, male, with the feet foremost. On examining the patient now, I found the membranes of another fœtus; these I at once ruptured, whereupon a *third* child, female, was expelled, the breech presenting. After its birth, I again instituted a careful vaginal examination, and was greatly surprised at discovering another bag of membranes, which I at once ruptured, whereupon a *fourth* fœtus, a male, head presenting, was delivered.

The first two children, I may here remark, were born alive, and cried, but did not long survive. The third and fourth children showed no signs of life at birth.

There were two separate and distinct placentas, each having two cords and two sets of foetal membranes connected with it. Some pressure on the uterus, and traction on the cords were needed to get the afterbirths away; but, from first to last, there was no loss of blood deserving the name of hæmorrhage.

This patient died of a low insidious form of puerperal fever about a fortnight after delivery, the friends obstinately refusing to follow the treatment I prescribed for her.

Although there was an interval of about eighteen hours between the births of the first and of the second foetus, yet the latter, contrary to what usually occurs after such a delay, was still alive at birth; but the two remaining foetuses, though expelled within a very short time afterwards, were quite lifeless when born.

As this was an extraordinary instance of reproductive power, I may, perhaps, be excused for giving some additional particulars relating to this woman and her family. I have already mentioned that she was only nineteen years of age, and that this was her first pregnancy. She was rather short in stature, and had a deep sanguine complexion, with dark brown hair and eyes. Her grandmother had twins, and her mother gave birth to twins on one occasion, and had besides eight other children. One sister of the patient was married and had borne a child. It appears then that the subject of this history came of a family which was endowed in no ordinary degree with the reproductive faculty.

DR. M'CLINTOCK said he had looked over the registry of the Lying-in Hospital, and found—as the author of the paper states—that there was only one year since 1757, in which a quadruple birth had occurred. It took place on the 30th October, 1788, in Dr. Joseph Clarke's Mastership. The name of the woman was Mrs. Hood, she was thirty years of age, and it was recorded in the register that she was an Irish woman. The children in this case were born alive and baptized, but subsequently died, though the day of their death was not mentioned. The woman herself left the hospital at the end of fourteen days. Dr. M'Clintock proceeded to say that there was in the Museum of the Lying-in Hospital a preparation of five children simultaneously conceived. The entry, which appeared in the Museum catalogue for that year (1839) was as follows:—"Five children simultaneously conceived; three separate ova; one single with its placenta; the others twins, each furnished with a common placenta; three months pregnant; miscarried in the summer, 1839." This woman was several times subsequently a lying-in patient of the hospital, on one of which occasions, he (Dr. M'Clintock) saw her. She was a stout sanguine woman, with fiery red hair, and the wife of a tailor. She used to allude, with no small pride, to her having once been pregnant of five children.

Saturday, February 17th, 1872.

DR. KIDD, President, in the Chair.

On Spurious, Feigned, and Concealed Pregnancy. By THOMAS MORE MADDEN, M.D., M.R.I.A., Examiner in Midwifery in the Queen's University in Ireland; Ex-Assistant Physician, Rotunda Lying-in Hospital, Dublin; Member of the Royal College of Surgeons in England; Licentiate of the King and Queen's College of Physicians in Ireland; and of the Faculty of Physicians and Surgeons of Glasgow; formerly Demonstrator of Anatomy, Carmichael or Richmond Hospital School of Medicine; Corresponding Member of the Gynecological Society of Boston, U.S.; Corresponding Fellow of the Edinburgh Obstetrical Society, &c., &c., &c.

SPURIOUS pregnancy is one of the most interesting subjects connected with obstetric medicine. The diagnosis between true pregnancy and pseudocyesis,^a including in this term cases of pretended as well as spurious gestation, as the occurrence of the symptoms of pregnancy without impregnation has been called, is oftentimes a matter of considerable difficulty and of the highest importance in a medico-legal, as well as in an obstetric point of view. Hence, the following particulars of several cases of spurious, feigned, and concealed pregnancy, together with the observations suggested by them, may, perhaps, be considered of some interest.

The comparatively large number of cases of spurious pregnancy which have come under my observation within the last few years leads me to believe that this morbid condition is of more frequent occurrence than is generally supposed. Nor is it confined to any class of society, as some writers think who speak of it as peculiarly affecting, idle and overfed, sterile, elderly, married women of the richer class. On the contrary, a large proportion of the cases of pseudocyesis, on the clinical study of which this memoir is based, came before me at the Dispensary for Diseases of Women, attached to the Lying-in Hospital.

The period of life at which pseudocyesis most frequently occurs is a point on which very eminent authorities differ. Thus the late Sir J. Simpson, speaking on this subject, in his *Clinical Lectures on Diseases of Women*, says:—"I feel pretty certain that the disease occurs at least as often during the first year after marriage as at any later period."^b Dr. Churchill has

^a Ψεύδς, a lie, and Κυήσις, pregnancy.

^b Medical Times and Gazette, September 3, 1859, p. 225,

seen a case of this kind in a patient, aged 17,^a and similar cases at a still earlier age have been recorded by Dr. O'Farrell and others. But my own experience leads me to agree with Dr. Montgomery, who, in his admirable work on the Symptoms of Pregnancy, stated that spurious pregnancy is most frequently observed about the turn of life, when the catamenia, becoming irregular, previous to their final cessation, are suppressed for a few periods.^b

SYMPTOMS OF PSEUDOCYESIS.

In pseudo-pregnancy we frequently find most of the ordinary symptoms of true pregnancy counterfeited with wonderful similarity. In such cases morning sickness following on suppression of the menstrual discharge is very commonly observed, and so also are enlargement of the breasts, the areola, and turgescence of the nipples, and glandular follicles of the breasts, and even secretion of a lactescent fluid from them. The abdomen generally increases in size, usually indeed much more rapidly than is the case in true pregnancy.

The enlargement of the abdomen in pseudocyesis may generally be traced to dropsical effusion into the peritoneal cavity, or to the tendency to fall into flesh about the period of "the change of life," giving rise to the excessive deposit of fat in the omentum; and thirdly, the increasing girth may be caused by indigestion, occasioning the distension of the large intestines, oftentimes to an almost incredible extent, by accumulated fæcal matter, and still more frequently by flatus, constituting what the poor in this country very graphically describe as a "windy dropsy."

In almost every instance of pseudocyesis that has come under my observation the patient asserted that she could distinctly feel the motions of the fœtus, and in one case where the woman had previously borne a number of children, she insisted, when suffering from spurious pregnancy, that she had never felt the motions of the child so strongly in any of her former pregnancies.

The patient's nervous system may, in these cases, present all the anomalous symptoms of pregnancy, such as longings, alteration in tastes, irritability of temper, neuralgic pains, &c., with great resemblance. Indeed, those who suffer from pseudocyesis have, as a rule, either borne children, and know all the symptoms by experience, or else have as it were "coached" themselves up on the subject, which now occupies their thoughts most prominently, and apply their knowledge to themselves with such a morbid concentration of ideas on the one topic that they become monomaniacs upon it, and ultimately deceive themselves as well as others.

^a Theory and Practice of Midwifery, 5th edition, p. 152. Dublin, 1866.

^b Signs and Symptoms of Pregnancy, p. 169. London, 1837.

No class of patients are more unsatisfactory to meet with in practice than those now under consideration. The duty of a medical man is seldom more unpleasant than when he is obliged to inform a woman, who for nine long months has cherished the belief that she is with child, who has communicated this intelligence to her friends, and made all the usual preparations for the expected little stranger, and who, perhaps, deceived by those anomalous periodic abdominal pains that sometimes occur in such cases, sends for medical assistance under the impression that labour is commencing, that she is not even pregnant. On the other hand, if the physician falls in with the patient's opinion at first without sufficient inquiry, and thus unconsciously makes himself a party in her self-deception, as soon as the true state of the case becomes obvious, he will incur an almost incredible amount of odium from the patient, who, not unnaturally, though often very unjustly, makes him the scape-goat for all the bitterness and vexation of which a woman's wounded pride is capable.

Even when merely called in by another physician to see a case of this kind in consultation, the consultant is placed in a very awkward position. He finds that the patient is not pregnant, and must say so. But as far as is possible, consistently with the patient's advantage, which, as in all other circumstances, is the first consideration of the physician, he should perform this duty so as to spare the reputation of his brother practitioner who may have pronounced her pregnant. In one case in which I was consulted, I found this a matter of no small difficulty, but, even then, I succeeded in disabusing the patient's mind of the belief that she was pregnant, and still, at the same time, maintained her confidence in her medical attendant.

CAUSES OF PSEUDOCYESIS.

The causes of spurious pregnancy are very numerous. Besides those already mentioned—namely, “change of life,” as it is popularly termed, dyspepsia, dropsy and obesity—pseudocyesis may result from a variety of other causes. Of these, two of the most interesting are molar pregnancy and uterine hydatidiform disease.

Molar pregnancy is a comparatively rare form of spurious gestation, although the poorer class of female dispensary patients frequently date the commencement of any uterine disease from the period at which they suppose they had what they term “a false conception.” On inquiring into such histories we generally find the case to have been one of an ordinary miscarriage. In two instances in which the so-called mole was kept for examination, I could discover nothing but a simple clot. A few examples of molar pregnancy have, however, come under my observation.

In the early period of supposed pregnancy there is no possibility of discriminating with certainty between molar and true pregnancy. Generally these substances are expelled from the uterus between the third

and fourth months, but if retained beyond the latter period the absence of the positive signs of pregnancy will of course determine the question.

Pregnancy may also be simulated by uterine hydatidiform disease, or cystic degeneration of the ovum. In a memoir published some years ago I discussed the symptoms, pathology, and treatment of this condition very fully.^a At that time I had seen only two examples of this very rare disease—two other cases of the same kind have since then come within my observation. In all these instances of uterine hydatidiform disease the patients supposed themselves with child, until they were undeceived by the expulsion of the hydatidiform mass from the uterus. In one case the woman strongly insisted (and she had given birth to living children previously) that she felt the motions of the child distinctly. The existence of these hydatidiform moles has been explained in various ways, but, like most others, I still think my own theory, as developed in the essay already referred to, the most plausible. In it I observed the theory most generally adopted at present is that hydatidiform moles are the result of the pathological degeneration or abnormal development of some one of the embryonic structures already existing in the uterus. But uterine hydatids sometimes occur under circumstances which prevent the possibility of their being connected with degeneration or abnormal development of any of the embryonic tissues—that is, in cases in which pregnancy never existed. In what light, then, are we to regard the occurrence under these circumstances? The answer to this is, I think, that we must ascribe such cases either to those constitutional changes which lead to the formation of true hydatids in other parts of the body, or else, and more probably, to morbid action set up within the ovary of an unimpregnated female, and which results in the production of hydatidiform disease in a graafian vesicle, and of its escape from the ovary into the uterine cavity, where it continues to increase in bulk until it excites uterine irritation and expulsive action.

Ovarian tumours may be confounded with pregnancy, or may co-exist with it. The diagnosis in such cases is, I think, comparatively easy, and may be arrived at by a manual examination of the abdomen, as well as by the state of the os and cervix uteri, which in ovarian disease is long and low down, while the uterus can be distinguished as a separate body from the ovarian tumour.

Pseudo-pregnancy may also be caused by ascites, by fibrous tumours of the uterus, by physometra, by hysteria and some other morbid conditions, into the consideration of which the limits of this memoir do not permit me now to enter.

Diagnosis of Pseudocyesis.—In the early stage of false pregnancy it is

^a On Uterine Hydatidiform Disease, or Cystic Degeneration of the Ovum. By T. M. Madden. 1868.

always a matter of considerable difficulty to pronounce an opinion as to the true nature of the case. But, however closely the symptoms of pregnancy may be simulated in the early months of pseudocyesis, the positive signs of pregnancy after the fifth month cannot be counterfeited. And, even from the very first, in spurious pregnancy it may generally be ascertained, on careful inquiry, that there is something unusual in the symptoms—either some essential one is wanting, or else the symptoms which belong to one period of pregnancy manifest themselves at another, and commonly earlier time than is natural.

Physical examination affords us comparatively little assistance in the diagnosis of pseudocyesis until the fifth month, and, as a rule, neither patient nor physician ever dream of the possibility of the case being one of spurious pregnancy at an earlier period.

With regard to the value of auscultation as means of diagnosis, I must confess myself to be somewhat doubtful. Even in the last month of gestation the fact of the sounds of the foetal heart and placental souffle not being distinguished on auscultation is, as I have shown elsewhere, no proof that the uterus may not contain a living child.^a Therefore, how much less reliable must this negative test be when employed, as in cases of this kind, at a much earlier period of gestation, or of spurious gestation.

Nor is the value of the positive evidence derived from the sounds of the foetal heart and placental souffle as certain as it is sometimes supposed to be. It is unquestionable that an experienced auscultator can pronounce on the existence of a living child *in utero* with all the certainty of actual knowledge from the auscultatory signs present. But all medical practitioners are not and cannot become experts in this special subject. Hence the error of fixing, as is done by some authorities, on a supposed pathognomic proof of pregnancy which is difficult to employ, and relying on which, to the neglect of other and more easily employed tests, opinions are sometimes pronounced in haste, which have to be repented at leisure. I am induced to make this observation by the fact that in two of the cases of pseudocyesis which came under my notice the patients were told they were pregnant, their medical attendant having in each instance persuaded themselves that they had discovered the sounds of the foetal heart on auscultation.

A manual examination of the abdomen with both hands will, if we succeed in relaxing the rigid condition of the abdominal muscles, which is so generally present in such cases, enable us to ascertain if there be any uterine enlargement or not, although not to distinguish between the enlargement caused by disease and that occasioned by pregnancy. To do this we must institute a digital vaginal exploration to determine

^a Maunsell's Dublin Practice of Midwifery. Edited by T. More Madden. London, 1871.

whether the conditions of the os and cervix uteri be what are usual at the corresponding period of pregnancy.

In those cases of pseudocyesis where the patient, being anxious to be thought pregnant, is either consciously or unconsciously contributing to the deception by making her abdominal muscles so tense and rigid that it becomes impossible to ascertain the size and position of the uterus by a manual examination, we may readily succeed in dissipating the phantom tumour, and overcoming the action of the abdominal muscles, by putting the patient under the influence of chloroform, and then examining her.

If the abdominal or uterine enlargement be occasioned by flatus or by physometra, percussion over the tumour will afford an easily applied means of discovering this.

CASES OF PSEUDOCYESIS.

CASE No. 1.—I was recently sent for in great haste to see a lady living a couple of miles from town, who was said to be in labour. I obeyed the summons at once, and on arriving at the patient's residence was met by the nurse, an old and experienced midwife, who expressed much pleasure that I had arrived in time, as she was sure, she said, that the child would be born before eight o'clock, it being then past seven o'clock. She added that it was a natural presentation, and that the os was nearly fully dilated. On entering the patient's room I found her in the usual obstetric position, lying on her left side, groaning loudly, and pulling hard at a strap fastened to the bed-post. She was a primipara, a delicate, hysterical looking woman, aged about twenty-eight, and about twelve months married. She had presented all the ordinary symptoms of pregnancy, except that she had a slight menstrual discharge, but paler and more scanty than usual, recurring at irregular intervals. She complained of the incessant tumultuous motions of the child, and stated that the overflow of milk from her breasts had spoiled all her clothes. On examination, however, I found the cervix long and low down, the os small and circular, the uterus presenting no sign of pregnancy, the abdomen very large and tympanitic, and the rectum enormously distended by accumulated fœces. Seeing that she was in a very nervous, excitable condition, I told her cautiously that there was nothing so urgent as she imagined in her case, as there was no sign of labour at present, and recommended her to call on me soon. Accordingly she came in to consult me within a few days. But when I hinted to her my opinion that she was not pregnant, and that it would be desirable for her to see another accoucheur in consultation, to ascertain the cause of the symptoms she suffered from, she became very indignant, pointed triumphantly to the lactescent fluid she squeezed from her breasts, insisted that she could feel the fœtal movements, and the next thing I

heard of her was that she had placed herself under the care of another physician, whom she wished to engage for her confinement, but who, being a very experienced and judicious accoucheur, had declined the engagement.

CASE No. 2.—August 12th, 1869.—L. P., aged twenty-five, an engine-fitter's wife, who was two years and three months married, and had one child, still-born, at full term a year previously; applied for advice at the dispensary of the Rotunda Hospital, as she believed herself to be in the seventh month of pregnancy, reckoning from the time of the supposed quickening, although she menstruated every month. She had suffered severely from morning sickness, and for the last three months believed that she felt the child's motions. There was a well-defined areola; the vagina was pale; the os and cervix uteri were hypertrophied and ulcerated; the abdomen was greatly enlarged, and resonant on percussion; the umbilicus was retracted, and the uterus was low down and small. Her bowels were habitually constipated, and her food of the coarsest kind. She was very hysterical, and nervously anxious about her condition. The enlargement of the abdomen, supposed foetal motions, and all the other symptoms in this case, were evidently caused by the distended condition of the large intestines, and by foecal matter and flatulence. She was purged freely, and ordered a mixture containing sulphates of iron and magnesia in infusion of quassia, and within a month's time regained excellent health.

CASE No. 3.—In midsummer, 1871, I was asked to see a lady residing in the country, in consultation, under the following circumstances. Mrs. —, aged forty-one, a stout, plethoric woman, having no family, though over ten years married, and who, till within the last year, had always menstruated regularly, and enjoyed excellent health, eleven months ago, for the first time, commenced to suffer from nausea and retching every morning. Shortly after her breasts began to enlarge and got painful, her appetite became capricious, her nervous system evinced considerable derangement, and obstinate diarrhoea then set in, which persisted up to the date of my visit. Her menses still returned every month, but instead of lasting for three or four days as usual, now only remained for a few hours each time, and were extremely pale and scanty. She consulted her medical attendant, who said that she was probably pregnant, but advised her to visit an accoucheur in Dublin. This, however, she refused to do. The period fixed on for the expected confinement was the end of April. Four months from the commencement of the symptoms just referred to, she began, as she said, to feel the motions of the child, which gradually became stronger and stronger, and the abdomen continued to enlarge. The doctor shortly

after this time imagined that he was able to detect the sounds of the foetal heart and placental souffle.

The time of her expected confinement at last arrived. The nurse took up her quarters in the house; some of the family came down from town to be present at the anxiously-looked-for event, and all her preparations, baby linen, &c., were completed. No sign of labour, however, manifested itself as week after week passed beyond the expected time. Her friends got tired out, her family returned home, and she herself became exceedingly nervous and desponding, as her mother had died of dropsy at about her present age. But still she insisted as strongly as ever that she could feel the child's motions distinctly. Such was the history of her case up to the time that I was asked to see her.

On examination I found the breasts slightly enlarged, but soft and flaccid. The nipples were somewhat turgid; there was a well-marked areola, and the glandular follicles around the base of each were prominent. The abdomen was about as large as that of a woman at the end of the ninth month of pregnancy. But the uterus was small, as I discovered, when, with some difficulty, I succeeded in taking her attention off for the moment, and overcame the resistance offered to any manual examination of the uterus by the abdominal muscles, which were tense, rigid, and arched. There was resonance on percussion, the large intestines being enormously distended by flatulence, the movements of which she had taken for those of the foetus. There was also a considerable quantity of fluid in the peritoneal cavity. The vagina was pale; the cervix and os uteri were low down, hypertrophied, and in a state of extensive granular ulceration.

Her chagrin, when informed that her preparations were not necessary, for the present at least, was naturally very great. Her medical attendant now agreed with me in recommending change of air, sea bathing, and a continuation of tonics and diuretics. Under this treatment the abdominal swelling and the symptoms of pseudocyesis disappeared with astonishing rapidity. The ulceration was treated with equal success, and her condition is now better than it has been for a long time.

CASE No. 4.—August 26th.—F. M., a clerk's wife, aged twenty-four, came to the dispensary to know what she should do to stop her changes, as she believed herself five months pregnant. She had had two children in three years since her marriage, and had on the present occasion suffered from all the symptoms she had before experienced when pregnant—morning sickness, enlargement of the breasts and abdomen, &c., and had fainted, as she always had done, as she said, "when she felt life in the child," a month previously. In this case, on examination, a large fibroid uterine tumour was discovered.

CASE No. 5.—M. T., aged 29, a plethoric woman, three years married,

had given birth to two still-born children. She was delivered of the last in January, 1869. Menstruation was then regular till June. She did not menstruate in July; complained of morning sickness, and imagined herself pregnant. At the end of August she had a profuse "discharge of the reds," in her own parlance, and came to the dispensary at the Rotunda Hospital for advice. I ordered her rest, cold astringent applications, and gallic acid with Dover's powder internally. She was desired to return in a few days, but she did not come back to the dispensary till February 19, 1870, when she told me that the treatment had completely checked the hæmorrhage. She still firmly believed herself to be pregnant, as her abdomen had been enlarging, her breasts had got full, and, as she asserted, there was milk in them, in proof of which she then squeezed a considerable quantity of a lactescent fluid from the nipples, which were prominent. The areola were well defined, and the sabaceous follicles around the base of each was as distinct as in any case of pregnancy. The patient, as usual in such cases, insisted that she could feel the child's motions, and that her sensations were in every respect similar to those she had experienced in her former pregnancies. For the last few days she had suffered from frequent micturition, especially at night, from tenesmus, and from irregular cholicky pains in the abdomen. She therefore believed herself very near her confinement, and had come to the hospital to obtain the usual admission ticket. On examination I found the abdomen very tense, and so protruded as to be fully equal to that of a woman at the end of the ninth month of gestation. But the appearance of the abdominal tumour was very different from that of pregnancy, being globular and uniform, not oval or pyriform as in pregnancy. The umbilicus was also retracted and the tumour disappeared when I succeeded in taking off the patient's attention by engaging her in conversation, so that I was able to satisfy myself that there was no uterine enlargement. The vagina was pale, and the cervix uteri was long and low down. She was exceedingly dissatisfied when told that she was not pregnant, and expressed her doubts in very indignant terms. She returned in a few days, however, and was ordered a cathartic draught, followed by a mixture, with sulphates of iron and magnesia in infusion of quassia, and under this simple treatment she rapidly regained her accustomed health.

CASE No. 6.—December 6, I was consulted by E. B., aged 40, who had been many years married, and had no family. Her menses had been regular, or rather profuse, ever since puberty, until about two years ago, when they ceased. For some years past she noticed that her abdomen was enlarging, but she did not pay much attention to this, until within the last eight months, when she rapidly became so large as to attract the observation of her friends, and to be incapable of following

her usual avocations, and she suffered much from loss of appetite and dyspepsia. Her neighbours pronounced her pregnant, and a medical man whom she consulted, after some examination, appears to have favoured their opinion. Acting on this, she made the customary preparations for her confinement, but at length entertaining some doubt as to her real position, she came up to Dublin to have the question decided. I found the breasts large, but soft and flabby, no areola, and the nipples very small. The vagina was narrow, and the os uteri, which was high up, was a small circular orifice not larger than the meatus urinarius. The abdomen was much enlarged, measuring thirty-five inches in circumference. This enlargement was caused by a solid uterine tumour, occupying the greater part of the abdominal cavity, but more particularly developed in the right hypocondriac and lumbar region than in the remaining parts of the abdomen. A distinct fremissment was perceptible on the right side, and also, though not so clearly, on the left. On applying the stethoscope a well-marked blowing sound was very plainly heard on the right side of the tumour, which was audible, though more of a cooing character, and not so distinct on the left side. This sound might very probably have been mistaken for the placental souffle, and thus misled the physician who first saw the patient. It is not necessary to pursue the history of this case further.

CASE No. 7.—Shortly before I left the Rotunda Hospital, a respectable looking girl, apparently about twenty years of age, a national school-teacher, was brought to me at the dispensary by her aunt, with whom she lived. Her friends had accused her of being pregnant, and her aunt insisted on her submitting to an examination. As her changes had ceased for some months, her abdomen was considerably enlarged, her appetite had failed, and she frequently suffered from retching. She herself denied the possibility of pregnancy. On examination, the hymen was found intact, the parts extremely small, no mammary signs of pregnancy, and it was ascertained that the enlargement of the abdomen was produced by a fibroid tumour.

Cases such as those I have just related should make us most cautious in pronouncing a woman pregnant or not, as well as in accepting obstetric engagements, until we have ascertained if our patient be really in the family way. Most patients come to their medical attendant merely to announce their condition, and to secure beforehand his services at the time of their expected confinement, and not to express any doubt or to submit to any examination; and as cases of pseudocyesis are comparatively unfrequent in ordinary practice, we are naturally apt, unless something unusual in the patient's age or symptoms attract our attention, to take it for granted that she is pregnant, on her own *ipse dixit*, and without further investigation.

PREGNANCY OBSCURED BY DISEASE.

Very closely connected with the subject of pseudocyesis, in which pregnancy is counterfeited by disease, or by art, is the consideration of those cases in which pregnancy simulates disease, or is concealed by the patient under pretext of it.

Pregnancy may exist under circumstances that seem very unfavourable to its occurrence, in persons of advanced age, and under conditions which render its recognition difficult and obscure. Hence, the necessity for much caution in giving expression to any opinion on this subject until we have made a careful examination of the patient.

Many cases, illustrative of the foregoing remarks, have come within my observation. One (Case No. 8) was that of a woman, aged thirty-eight, who had been married eighteen years and had no family. She had never had any symptom of pregnancy. Her menses were irregular, and she suffered from extensive granular ulceration of the os and cervix uteri, for which she had been under my care as an extern patient at the dispensary of the Rotunda Hospital, for a considerable time, without any substantial benefit, as the disease would at times almost completely subside under the treatment adopted; she would then absent herself for some time, and return as bad as ever. I induced her at last to come into the chronic ward of the Hospital. She was suffering at the time of her admission from menorrhagia, to which, as is common in such cases, she was occasionally subject. The os and cervix uteri were congested and in a very angry looking granular state. The granular surface was cauterized with nitrate of silver, and cold douching was employed night and morning. Under this treatment the bleeding was checked, and for several days she progressed favourably. In about a week from the time of her admission, however, I was called up late one night to the chronic ward, being told that Mrs. R. had a bad attack of cholic, and that the hæmorrhage had also returned, and within a few moments of my arrival a considerable gush of blood escaped, and with it was expelled a very perfect ovum of between the second and third months.

CASE No. 9.—A lady, who having been married at a very early age, had three children before her twenty-first year, and then had no sign of pregnancy for over seven years, was attacked by fever, on recovering from which she was affected by violent hysteria; this ultimately passed into acute hysterical mania, necessitating her being placed under restraint for a short time. Before this step was taken her menses having suddenly ceased, she was treated by the strongest emenagogues in very large doses, and persisted in for a considerable period for the purpose of restoring the catamenia, very providentially however without any effect. After the complete subsidence of the cerebral symptoms her general health became excellent, and a short time after, whilst travelling, her figure, which was

naturally slight, suddenly altered, and she very rapidly increased in size. She now, for the first time, suspected that she might possibly be pregnant, and soon after her arrival in this city, consulted an eminent physician, who, misled by the complication of hysteria in the case, and by the fact that until six weeks previously there had been no abdominal enlargement whatever, concluded, as indeed was very natural under the circumstance, that the case was one of hysteria, and that the sudden abdominal enlargement was due to the distension of the large intestines by flatulency. He therefore told her that she was not pregnant. He, however, called in an accoucheur to visit her, who recommended a midwife to be sent for, and the following day the child was born.

MEDICO-LEGAL ASPECTS OF PRETENDED AND CONCEALED PREGNANCY.

Pregnancy may be feigned for the purpose of committing fraud or escaping punishment, and it may be concealed to avoid disgrace or with the intention of perpetrating crime. The former cases are I believe much more common than is generally supposed, although they are less commonly obtruded on the notice of medical practitioners than the latter. Still their occurrence is a matter the possibility of which should not be forgotten, especially by those engaged in obstetric practice. I had written out the notes of one attempted case of this kind that came within my own experience, but for reasons that need not be dwelt on have determined to omit it from this paper.

I need not enter at length into the legal aspects of this question, as these are fully discussed by writers on medical jurisprudence.

It is sufficient for our present purpose to state that two most important topics fall under this head. First as regards the existence of pregnancy, which may be pleaded in a criminal case as a bar for punishment, as the English Common Law, founded on the Roman Law, which exempted a pregnant woman from punishment until after her delivery "*Quod prægnantis mulieris damnatæ pæna differatur quoad pariat*," provides that if a woman be capitally convicted and pleads her pregnancy, though this is no cause to stay the judgment, yet it is to respite the execution till she shall be delivered. In this case a judge may direct a jury of twelve matrons, or discreet women, *de circumstantibus*, to be empanelled to try "whether the prisoner be with child of a quick child or not." For Blackstone distinctly states what appears to be still the law of England, that "barely with child, unless it be alive in the womb, is not sufficient."^a If she be found quick with child she is respited until she is either delivered, or proves by the course of nature not to have been with child at all, otherwise the sentence will take effect.

If cases such as I have recorded, in which not only women who had

^a 4 Blackstone's Commentaries, p. 395.

no motive for practising any deception, and who had before borne children, went on throughout the course of pregnancy, until almost the moment before delivery, without ever suspecting themselves with child, and in which qualified medical men, on examining such women, denied that they were pregnant, as well as the more numerous cases in which the contrary error was made, have any value whatever, it is that they show the absurdity of the law in committing the solution of a question of such gravity as this, which may involve the issues of life and death, to any twelve matrons, however ignorant, who may happen to be present when this plea is raised. Nor can anything more at variance with common sense and modern physiological science be conceived than retaining the barbarous distinction in such cases between a fœtus after the sixteenth week when *quickening* was supposed to occur and one before that period, as the veriest tyro in medical study should know that the living embryo at the moment after conception is as certainly living, or quick, as the fully matured man in the prime of his manhood, and that the crime is as great to destroy the one as the other.

Within the last few weeks this very issue was raised in the case of Christina Edmunds, who, on being found guilty of murder at the Old Bailey, pleaded pregnancy in arrest of judgment, and being found to be "not *quick* with child" by the jury of matrons who were empanelled to try this question, was sentenced to death, and would have been accordingly executed had she not been respited on other grounds. There can be no doubt that cases have occurred in which pregnant women have been executed on the faith of the verdict of "not quick with child" of a jury of matrons. An execution under such circumstances is unquestionably the judicial murder of the child.

It is certainly full time that measures should be taken to alter the existing most barbarous law on this subject, and this should be done before the unborn offspring of another woman is sacrificed as a victim to the ignorant inhumanity of our penal code. For, as experience has proved in similar cases, it is by no means impossible, though improbable, that the wretched woman to whose case I have referred—Christina Edmunds—might have been truly pregnant, despite the verdict to the contrary of twelve ignorant women, to whose decision the most difficult question in medical diagnosis is left by the law.

CONCEALED PREGNANCY.

Concealed pregnancy is of much more frequent occurrence than feigned gestation. This subject, though one of great practical interest, is altogether too wide a one for full consideration within the limits of a paper such as this, which is already, perhaps, over-long. But still I would desire to call attention to the fact that the practice of concealing pregnancy, with the intention of committing child murder, or of procuring

abortion, and especially the latter, is, I fear, becoming of late years more common in this city than was formerly the case. The reasons for this are the increasing proportion of illegitimate births, owing to circumstances still in operation arising out of the famine period, since which the proportion of the married to the unmarried, previously greater in Ireland than in almost any other country, has been considerably diminished. One result of this is that illegitimate births are more frequent than they were in Ireland. But this increased proportion of illegitimate births is by no means a full measure of the extent to which the evil to which I have referred has gone. For of late years, with the deluge of cheap bad literature which is poured into this country, and which circulates chiefly amongst the class that constitute the majority of the unmarried patients of the lying-in hospitals, a still greater evil has become familiarized to the oftentimes badly-reared and sorely-tempted victims of seduction, who too often seek what they falsely believe to be a safer mode of escaping the penalty of their error. Hence it now becomes more than ever necessary for every medical practitioner to be prepared to meet with cases of concealed pregnancy and attempted abortion under various disguises, and thus be able to detect and frustrate such crimes. So often have I detected pregnancy in patients who applied for emenagogues under the pretext of simple amenorrhœa, and who were most indignant when any doubt was thrown on their statements, that I never under any circumstances prescribed any emenagogue at the dispensary until I had convinced myself that the case was a fit one for their administration. In other words, the safe rule in such cases I believe to be just the reverse of the legal maxim, and we should, in cases of amenorrhœa, with the history and causes of which we are not perfectly acquainted, treat the patient as though she was pregnant until we are satisfied that she is not so. But I need not add that we should do this without expressing any suspicions that may, after all, be unfounded, and simply order some placebo when pressed to prescribe emenagogues, until, by a little observation, we have time to ascertain the true state of the case.

A considerable number of instances of concealed pregnancy have from time to time come under my observation. Amongst these I may mention that of a young country girl, who succeeded in persuading a number of experienced medical practitioners in different parts of the country into the belief that she was suffering from an ovarian tumour, she being at the time advanced in pregnancy, the existence of which was not even thought of. So far was the deception carried that a treaty was entered into with an eminent surgeon for the performance of ovariotomy. But as the fee required appeared to the girl's relations to be very large, they got her admitted as a patient into a metropolitan hospital, although they could well have afforded the necessary expense of medical treatment. She was admitted into the hospital as a suitable case for

ovariotomy, but a few days after admission the true nature of the case being detected, she was transferred to the Rotunda Hospital, where she was delivered the day following her admission. Even when labour set in she still, however, persisted in denying that she was or could be pregnant; and it was not till the child was born that she confessed the truth.

Cases such as those just related show the importance of every medical practitioner making himself thoroughly familiar with the diagnosis of all the cases in which pregnancy may be feigned, concealed, or counterfeited either by art or by disease.

After a few remarks by the President and Dr. Atthill, a paper,

On the Use of a Hemp-Saw for the Excision of Polypoid Growths, was read by ALFRED H. M'CLINTOCK, M.D., F.R.C.S.I., Ex-Master Lying-in Hospital.

I VENTURE to bring under the notice of the Society, a mode of excising uterine and vaginal polypi, that would seem to be simple and effectual, and, at the same time, to possess a wide range of applicability. To claim for it a superiority over all the various means at present employed for removing these growths, would be presumptuous, with my present limited experience of its use in actual practice. Nevertheless, enough can be said in its favour to prove that it is well deserving of a trial; whilst I am not without hope that in many cases it may be found more feasible than any other mode of operating.

No doubt most of those present recollect a paper read here last February, by the President, on decapitation, in certain cases of presentation of the arm. In that paper he described the operation recommended by Heyerdahl, of Bergen, for severing the head from the trunk, and practised by him, Dr. Kidd, which operation consists in sawing the neck through with a chain or cord of wire or hemp.

Since reading Dr. Kidd's paper in the Dublin Journal (for I had not the good fortune to be present when it was communicated), it occurred to me that some such method might often be advantageously employed for the extirpation of uterine polypi. Feeling persuaded of this, I became very anxious for a case on which to make trial of the plan. Such a case recently presented itself, and I hasten to lay before you the results. A married lady, aged about forty, who had borne four children, the last of them four years ago, consulted Dr. Symes, of Kingstown, and subsequently myself, in consequence of profuse and frequently repeated hæmorrhages from the vagina. These had been going on for many months, and had very much lowered, and in other ways impaired her health and strength. On examination we discovered the presence of a large polypus in the vagina, and as I entirely concurred with Dr. Symes in the propriety of the tumour being extirpated, we met for that purpose, at the lady's house, on the 10th January. The polypus was about the size of a turkey's

egg, and was high up in the vagina. Its neck was fully as thick as a man's thumb, and was closely embraced by the os uteri. There was a foetid discharge from the vagina, and the manipulation of the tumour caused some blood to escape.

Whilst I retracted the perinaeum with the duckbill speculum, Dr. Symes seized the tumour with a vulsellum, and drew it down till it just appeared at the vulva. I then carried around its neck a loop of twisted silk fishing line attached to a Gooch's double canula; and having carefully adjusted the ligature we slipped up the instrument upon it, as high as it would go, at the same time drawing the ligature tight. Now, whilst Dr. Symes held the canulae firmly in their place, and also the tumour, by means of the vulsellum, I drew the ligature to and fro alternately with each hand, keeping a pretty tight strain upon it all the time. In about thirty-five seconds, it cut through the neck of the growth, which Dr. Symes then drew away with the vulsellum. I now exhibit the tumour and the instrument. The former weighed three ounces and a-half, and the diameter of the excised section of the pedicle was one inch. The tumour is what Dr. Matthews Duncan would call a "true polypus," as it possesses a well marked capsule, part of which, however, has become ulcerated. It is very fibrous in structure, but not so dense as we sometimes see uterine fibroids to be. Vessels traversing the pedicle are distinctly traceable.

The operation caused no pain beyond that which the dragging down of the tumour gave rise to, and not a dessert-spoonful of blood was lost from first to last. On examination with the speculum immediately afterwards, the remains of the pedicle had retracted within the os, so as to be scarcely visible; an application of carbolic acid and glycerine was made to it.

Nothing could have been more satisfactory than this operation. It was facile, expeditious, and unattended by any pain or bleeding.

But, then, the case was a most favourable one, and I have no doubt the removal of the polypus could have been effected with equal success by the *ecraseur* or the scissors.

I have learned from Dr. Symes that the lady's recovery was rapid and complete, and that the metrorrhagia has entirely ceased.

The ligature was applied and used through a Gooch's double canula, whereby the vagina was entirely protected from any injury which the sawing of the cord might have inflicted. In operating on a tumour higher up, or on a large tumour, or on one within the uterus, it would be desirable to have the end of the instrument somewhat curved, so as to assist in preventing the cord from being chafed or worn by the friction against the distal orifice of the canula.

Inasmuch as a closely twisted silk or hemp cord is capable of sawing through the neck of a foetus, there can be no doubt of its being adequate to cut through the pedicle of any polypus that we ever meet with in the

uterus or vagina. This in itself is no small advantage, as I have often seen the iron wire and the wire rope of every degree of thickness, when used with the ecraseur, break on fibrous tumours, thereby causing much trouble and delay in the performance of the operation.

But, whilst I freely admit that the hemp ligature, when in actual use, might possibly give way (as it did with Dr. Kidd, though not till the neck of the foetus was cut nearly quite through), still I do say that we always have it in our power effectually to guard against such an occurrence—simply by employing a good long piece of cord, and from time to time, as we see fit, drawing down one end of this ligature, so as to bring a fresh portion of it into contact with the surface we are operating upon. This, you perceive, can be done without the slightest difficulty, delay or disturbance of the operation.

With a view to test the cutting power of the twisted hemp, or silk cord, used as a saw, in the manner before described, I made some experiments, which are, perhaps, worth mentioning here:—

Experiment 1.—With the kind assistance of Dr. Cranny, I operated on the neck and on the thigh of a full-grown foetus, two or three days after death. I put a ligature of strong twisted silk fishing line around the neck, and whilst Dr. Cranny held the foetus, I commenced sawing. In about half a minute the ligature had cut down to the spine, and partially through it. At this stage, the ligature gave way twice, so I substituted a piece of strong whip cord, and with this the decapitation was completed in about a minute. On examination we ascertained that the cord had cut not through the intervertebral substance, but right through the body of the 4th or 5th cervical vertebra.

Experiment 2.—We next put the whip cord around the upper part of the thigh. It very quickly sawed through all the tissues down to the femur; but only a very slight impression could be produced on the bone, so that after some minutes trial it seemed fruitless persisting any longer in the attempt to cut it across. The periosteum was divided, however, and a slight groove marked the site of the ligature on the femur.

Experiment 3.—The animal tissue which, next to bone, would seem most calculated to resist the action of the cord, is ligament or tendon. I therefore tried the effect of the fishing line (the same as that which broke in the vertebra), on the strong hamstring tendons of an ox, near to their insertion at the hock. It required nearly ten minutes to cut through this structure, and strange to say, this feat of “hemp-sawing” was accomplished without the ligature once giving way. When the enormous strength of this tendon—by far the thickest in the animal’s body—is considered, the result of this experiment may well excite surprise. I doubt if any ecraseur could have done as much. I tried a chain ecraseur of my own on the same tendon, and worked it till the screw got so tight and required so much force for its rotation, that I felt sure that the snapping

of the instrument would have been the inevitable result of pushing the trial any further. Even with this high strain (which far exceeded any I had ever subjected the instrument to), the chain had made only a very slight impression on the tissues embraced in its grip.

With all these facts in view, then, I think it may safely be affirmed that the hemp-saw is adequate to cut through the structure of any uterine or vaginal tumour coming before us for operation.

To encircle a neck of a tumour of large size, or high up, with a stiff metallic ligature, whether of single or twisted wire, is often a matter of considerable difficulty, even in the most expert hands. To assist in accomplishing this object, Dr. Atthill has supplemented the wire ecraseur of Braxton Hicks, with Gooch's canulæ, in order to carry the wire ligature round the base of the tumour. "This," he observes, "can always be accomplished when a silk or hempen ligature is used, but it is very difficult, indeed, to carry a stiff wire round a large tumour."

The extreme difficulty of surrounding an intra-uterine polypus with a wire or chain, induced Dr. Marion Sims to invent an intra-uterine ecraseur, to borrow Dr. Atthill's language, is a "marvel of ingenuity, but very complex, and in practice has proved a failure."

The hemp or silk ligature then undoubtedly possesses this advantage over the wire ligature—viz., That in many cases its application around the neck of the tumour is more certain and easy of accomplishment.

Furthermore, whilst the hemp-saw is not more likely than an ecraseur to include a portion of the adjacent tissue, it certainly is less likely to draw any within its grip, an accident which has happened to myself in using the ecraseur, and which has led to alarming results in operating upon the cervix, of which Dr. Marion Sims gives a striking example, when the peritonæum was laid open under his own hands.

Whilst claiming for the hemp-saw some advantages over the ecraseur in the actual performance of excision, I nevertheless cannot take upon me to assert that it may not entail in its consequences a greater likelihood of hæmorrhage or of pyæmia. These are points, however, which experience alone can satisfactorily decide, though I confess to having little doubt in my own mind, as to the result with regard to them.

In case the tumour be very large, or high up, the canulæ should have a good length—at least nine inches, independently of the stem, or socket; and they should also possess considerable strength. In all cases the orifice at the distal end of each canula should be finished off with a well-rounded margin or edge that cannot possibly cut or fray the ligature. I have seen two instances where failure resulted from the instrument being deficient in these respects. In each of them the polypus was intra-uterine; in one the canula snapped across as the ligature was being applied, and in the other the ligature speedily chafed and broke in the act of cutting the neck of the tumour.

In speaking of the ligature, or cord employed for sawing purposes, you may have remarked I used the term "twisted," as I believe that the hard rough surface (or edge, so to speak) presented by a closely twisted cord, whether silk or hemp, very materially contributes to its cutting power, where friction is combined with pressure; and such is the *modus operandi* of the "hemp-saw."

The PRESIDENT said:—As Dr. M'Clintock had alluded to the use of a hemp-saw for decapitation, he took that opportunity of exhibiting to the Society the result of the use of that saw. He had before him the body of a child that he delivered on last Tuesday week by decapitation in the manner which he suggested in his paper read last session. He had not then tried the method of passing the cord that he ventured to suggest, but he tried it in this instance, and it answered his expectations very fully. This was a case of transverse presentation. When he saw her the woman had been many hours in labour, the right arm was hanging out of the vulva and the left lying in the vagina. The patient was under the care of Dr. Arthur Ringland, who sent for him. When he arrived Dr. Ringland passed his hand into the uterus and tried to turn the child, but the uterus was so firmly moulded round it that, without using more violence than was prudent, he could not seize the feet. He (the President) then tried, but could not reach the feet; he first tried with the right and then with the left hand. Finding that the child was dead, instead of persisting in the effort to turn it, he determined to decapitate. He passed a cord round the neck by means of a catheter—a silken cord—the very same he had used on a previous occasion, and proceeded to divide the head from the neck. In a minute and a half section was made. One observer said he was rather over a minute, and another that he was a minute and a half in performing the operation. The total operation lasted 20 minutes from the time he began until it was completed. He cut through the body of the last cervical vertebræ and through the transverse process. He shifted the ligature as he worked it, to bring a new part of it into play. After the head was separated he drew the body out by the arm, and then seized the head with a forceps and brought it down until he got his finger into the mouth, and delivered the head without any difficulty whatever.

DR. BEATTY said that any one who had gone through the toil and difficulty of the old operation of evisceration in a case of transverse presentation, where the arm was down in the vagina, the child a large one, and no possibility of turning, would rejoice at the introduction of such an operation as the President had brought before the Society, and which had been employed and further utilized by Dr. M'Clintock. The simple operation by means of a cord being substituted for the fearful

operation of evisceration, was one which those who had gone through the latter must rejoice at, and which the gentlemen of the present day ought to be very happy at having placed within their reach. He had himself toiled for an hour or an hour and a half over the operation of evisceration—the most odious and disgusting in obstetric practice. The simplicity of Dr. Kidd's operation seemed to him almost miraculous contrasted with that he had alluded to, and it was one of the greatest boons conferred on midwifery in modern times. Its application to cases of tumours was a matter of considerable importance. Dr. M'Clintock had alluded to the possible occurrence of hæmorrhage, which had struck him also. The rapidity of the cutting through might leave the possibility of hæmorrhage, which was avoided by the slower process of ecraseur, the crushing effects of which were calculated to prevent bleeding. However, he thought Dr. M'Clintock's method was to be looked upon as a considerable improvement in the process of removing vaginal or uterine tumours, but that of decapitating the child in utero was one of the greatest additions that had been made of late years to obstetric knowledge.

DR. RINGLAND thought the Society could not be too much indebted to Dr. M'Clintock for the communication he had made. It had added another laurel to the many he had gained at that Society and at the hands of practical men. He felt that the operation suggested was a perfectly available one, and, at the same time, the idea that struck Dr. Beatty, struck him as the paper was in progress, but he found the same idea had also come across Dr. M'Clintock's own mind. He believed the occurrence of hæmorrhage was by no means an impossible event in the progress of this operation. They must bear in mind, in contrasting the two operations that there was a *de facto* different process undergone. The one was a distinct sawing operation, producing no effect on the blood vessels themselves, whereas the other was a crushing process; and they must bear this in mind also, in serious cases more especially, that it was an operation in the cavity of the uterus, where the tissues were to a great extent beyond their control. Dr. M'Clintock recalled to his memory a case which occurred under the President's notice and his (Dr. Ringland's), within a few weeks past, where an enormous tumour presented itself, and when by the ordinary process by the ecraseur it became impossible to remove even portions of it. Every time the ecraseur was tightened it slipped off the tumour. The operation was postponed to the following day in consequence of the neck of the uterus not being sufficiently dilated by sea-tangle; and whether it was the handling the patient was subjected to or not, he could not say, but she quickly sank. That case directed attention to the possibility of adopting some means whereby an enormous tumour, such as existed in

that instance, might have either the wire of an ecraseur or some other means applied to it; and they came to the conclusion that in the event of a similar case arising, where the tumour was of enormous size, where there was an utter difficulty of passing the wire round it so as to embrace any portion of it, an instrument somewhat analogous to the instrument of Pajot, might be employed for the passing of the means whereby the cutting process was to be effected—whether by wire or a hair-pin ligature—an instrument such as that used for plugging the posterior nares. But, thinking over the matter since, he believed that the means employed by Dr. Kidd in decapitation might equally apply to any of those cases, namely, the employment of a large-sized elastic catheter into which Simpson's sound was introduced. The ligature might be either carried on through the catheter, or it might be attached to the end of the catheter and drawn back over the object to be cut through. He regretted that he did not see the entire of the operation performed by Dr. Kidd. He did not see the way in which the ligature was applied, having been present during the cutting operation only, and he believed that it was done in little more than a minute. He believed that the practitioners of the present day would derive a large amount of benefit from that operation, escaping, by its means, from the necessity of performing evisceration. A case of that kind occurred within his own practice. The operation was commenced, and it was eventually terminated by fracture of the spinal column. But decapitation by the means suggested by Dr. Kidd, was far more easily effected. The facility with which the entire body was extracted, the rapidity of the operation, and the favourable result, clearly proved that the operation was not only feasible, but one that could be easily and safely performed.

DR. CRONIN could not accept the idea that decapitation, involving, as it does, the risk of leaving the foetal head in the uterus, was a good or safe operation. He had frequently seen the operation of evisceration performed, and it was invariably a safe and easy operation. He had seen cases brought into the Rotunda Hospital, where the patient had been in labour for days, the arm protruded from the vagina, the uterus moulded firmly over the body of the foetus, and the operation of turning evidently unsafe, if not impossible; he had seen evisceration performed in these cases, and all of the patients made easy and quick recoveries. He could well understand that Dr. Kidd had conducted this operation in the most perfect manner, and it had turned out well; but they must consider the difficulty in which Dr. Kidd or any other practitioner would be placed if the os uteri contracted on the head too quickly to allow the forceps to be applied, and the practitioner had to retreat leaving the head in the uterus. This had occurred more than once, and it might happen again, especially where there was a large head. If, in such a case, the

body being severed, the practitioner had no power to draw the head down, what was he to do then? As to the use of the hemp-saw in removing polypi, he had no doubt it was useful, but, at the same time, he had seen polypi taken away by Gooch's canula and a tight ligature, and all had gone on well.

Dr. ATTHILL said that every one who had had experienced the difficulty of encircling with a wire an intra-uterine tumour would be grateful to those who lessened the difficulty of dealing with such cases. He had himself suggested a modification of Gooch's canulæ, but he did not think he would use it again. It did very well if a hemp ligature were sufficient, but that was seldom the case, and the difficulty of passing a wire around the tumour with the canula was enormous. He saw Dr. Denham succeeded with his (Dr. Atthill's) instrument in a case of extra-uterine polypus, but the attempt to pass a wire round an intra-uterine polypus with the canulæ was a matter of serious difficulty. Dr. M'Clintock's method was ingenious. He had had the good fortune to be present at a trial of it, and although the operation was not altogether successful, it gave him a fair idea of it. It was a case of intra-uterine polypus low down in the uterus. The ligature was certainly passed round the tumour, but after a very few seconds of sawing, the ligature gave way. That was the real difficulty to contend with. There was great friction on the cord, and until some means of lessening this were devised, the liability to its breaking would be very great. It would be a matter of difficulty to cut through a thick pedicle without the cord giving way, unless the canulæ were modified in such a way as to cause the two ends to diverge somewhat, thus lessening the amount of friction. He was not himself afraid of hæmorrhage, and as to pyæmia he did not think there was any additional risk of its following the use of the hemp saw. He disagreed with Dr. Cronin as to the dangers likely to follow the attempt to remove polypi by the old and now discarded method introduced by Dr. Gooch. He had never lost a patient by practising it, but more than one had been at death's door, and they knew from the statistics of practices more extended than his, that the mortality, after the application of the ligature in the old method, and the tightening it day by day, was great indeed. He entirely coincided with Dr. Beatty as to the advantage of decapitation, which he thought would make a complete revolution in the practice in case of arm presentations. His knowledge of evisceration was very unfavourable. It was a most tedious, horribly disgusting operation. On one occasion he was two hours before he could get his hook so fixed as to get down the body. He should run the risk of having the head retained in utero, which was only possible, rather than encounter the difficulty and trouble of evisceration, which was certain. If the os contracted somewhat, they could, by the

use of Barnes' dilator, open it so as to perforate the head in utero, or seize it with the forceps.

DR. FRANK THORPE PORTER said that on one occasion he was called to a case of twins. The first was born without difficulty. In the case of the second child one arm was outside the vulva and the other in the vagina. He tried to introduce his hand, but it was of no use, and as a last resource he would have tried evisceration, but that the friends of the patient had some scruples against it. Within an hour spontaneous version actually took place, and the child and the mother were now living. In that case the funis was not pulsating. Therefore the non-pulsation of the funis was not a proof, as some said it to be, of the child's death.

DR. T. MORE MADDEN said that he did not purpose to make any general observations on the very valuable paper just read by Dr. McClintock. But as during the discussion which had arisen upon that paper, great prominence had been given to one topic, and opinions had been expressed upon it by the previous speakers, in which he could not coincide, he could not refrain from giving expression to his views on the question at issue. He referred to the subject of decapitation by means of the ligature as a substitute for embryotomy as proposed in certain cases as a last resource by Dr. Kidd. Whilst he (Dr. Madden) agreed that in the hands of an operator so skilful, experienced, and conscientious, as every one must know Dr. Kidd to be, and one who would not employ any embryotomic operation until every other means that science could suggest had been most patiently and most skilfully tried, but had failed, and not until the child had been probably long dead, the operation might be an improvement on embryotomy. But although he knew that Dr. Kidd would act thus, he (Dr. T. Madden) could not admit that all the practitioners into whose hands this new and facile method of decapitating a foetus *in utero* might come would be as patient, as skilful, or as conscientious as Dr. Kidd. Therefore he could not join in the delight with which some previous speakers had welcomed the new operation. He thought that embryotomy was an operation so awful and so horrible that nothing should be done to familiarize its use or to substitute for it another operation more facile, less wearisome to the operator, and less revolting to the spectator, but performed, nevertheless, with the same object. He thought that the more horrifying such an operation was the less frequently would it be resorted to. Even in cases of cross-birth and difficult labour, where every circumstance seemed unfavourable to the birth of a living child, he (Dr. M.) had seen birth given to one. For proof of this he referred to a case of compound presentation which he had brought before that Society two years ago, in

which the funis was prolapsed and the membrane ruptured for twenty-four hours before the patient was seen, and yet in which he succeeded in performing version and delivering the woman of a living child with safety to herself, and this in direct opposition to what is regarded as the recognized rule of practice in such cases, as he was obliged to use a degree of force and perform version under circumstances which are held by British writers to forbid any hope of recovering the child alive. He therefore thought that it was impossible to use too great a degree of caution before arriving at the conclusion that the child could not be born alive, and that it was by no means desirable that embryotomic instruments should be improved and rendered more easy of use. On the contrary, he thought too much attention could not be given to the improvement of the means of delivering living children from living mothers, with safety to both, by the forceps or by other means.

DR. DENHAM.—With respect to decapitation he thought they owed a debt of gratitude to the President for bringing the subject before them. At the same time he believed the operation to be by no means so free from danger as some appeared to suppose. The child which the President exhibited that evening, was a small child, and the woman might have had a well-formed pelvis and a well dilated os; but he could well conceive a case in which it might be attended with tenfold the difficulty that Dr. Kidd encountered. In many of these cross births the child was unusually large. If they took a case of first labour, a large child, the parts not well dilated, the waters drained off for hours, he could conceive a great amount of difficulty in taking away the head. He had seen great difficulty in removing the head of a premature birth. Nay more, in endeavouring to pull down the body of a decomposed child, he had seen the os close on the neck, and the head left behind, and he had experienced very great difficulty in the removal of such a head. They would therefore require a large amount of experience before they could pass a decided opinion as to this operation. He could not coincide in the remarks of Dr. Madden. If a cross birth took place, and the practitioner came to the deliberate conclusion that he must deliver the woman, the quickest and easiest method of doing so was what he was bound to adopt. Of course it required considerable deliberation. It was a painful thing to have recourse to such an operation, even when the child was dead, and he believed few of them would attempt to eviscerate or decapitate, knowing that the child was living; but having come to the conclusion that it was necessary to deliver, the easiest and the safest method was the one they were bound to adopt. The safety of their patients was the first matter to be considered by obstetric practitioners. Their own toil, disgusting though it might be, was of small importance compared with the life of their patients. He regretted that they had heard nothing more practical as to Dr. M'Clintock's

paper. At present it was mere theory, for, with the exception of a single case, they had nothing practical to guide them. He had seen it tried without success on a large uterine polypus. There was a great distinction to be drawn between the cutting off the head or thigh of a child, and the removal of a polypus in the uterus, and Dr. Kidd's plan, while adapted for the former, might not prove so efficient in the latter operation. He should like to ask Dr. Porter how he knew that in the case he had mentioned the cord in utero was not pulsating? Was it not so placed that he could not touch it?

DR. PORTER said the cord was not beyond his reach; he could have felt it if it were pulsating, and he satisfied himself that it was not.

DR. M^cCLINTOCK, replying, said he must first express his admiration of the operation which the President had introduced to their notice. It came to our aid in the most difficult and embarrassing class of cases to be met with in midwifery practice. Some of his most painful obstetric recollections were connected with cases of evisceration. It was not a well founded objection against the operation to say that it was easy and safe, provided it was capable of effectually fulfilling the intention with which it was undertaken. Of course he took it for granted that no man of principle, or of information in his profession, would ever have recourse to such an operation, until he had made full and satisfactory trial of turning, and then, if nothing else could be done, it became his duty to perform evisceration or decapitation. Surely the evil of such an operation fell far short of the patient dying undelivered, or with ruptured uterus. There were some grounds to apprehend that there might be difficulty in extracting the head after the removal of the body; any one reading Smellie's *Midwifery* must have his mind filled with apprehensions on that point; it seemed to have been to Smellie a regular bugbear. When the removal of the head was undertaken without delay, there would seldom be any difficulty in doing it; but he did not think the difficulty, whatever it might be, was to be put in comparison with the danger and trouble of evisceration.

If there were any merit in the method of removing vaginal and uterine tumours, which he had brought before them that evening, it had grown out of Dr. Kidd's paper, the reading of which put the idea into his (Dr. M^cClintock's) mind. There were two points which he wanted to establish, and he thought he had succeeded in doing so. One was, that a ligature of this kind—"a hemp-saw"—was capable of cutting through any structure they were likely to meet with in uterine tumours. The other point he endeavoured to prove was, that this contrivance of Gooch's canula, they could with more facility carry a ligature round the neck of a polypus, when it

was very high up, or very large, than if they used a wire ligature. In one case alluded to the canula broke, and hence the importance of using a very strong canula. The failure of the operation was owing to the imperfection of the instrument. In the other case (that alluded to by Dr. Denham), the ligature was successfully carried round the neck of an intra-uterine tumour; but the edges of the orifices of the canulæ were so sharp that they cut the ligature across in a moment. Therefore, in the construction of the instrument for this operation, the eyes should be rounded off, so that they should not fray the cord. As to the practical value of the operation, it could be decided only by clinical experience. It supplied us, however, with an additional resource in the treatment of these tumours, and one which might occasionally prove more suitable than any other operation.

The President remarked that if the practitioner had a cephalotribe beside him he need not fear the risk of the head remaining in the uterus.

The Society then adjourned.

NEW PREPARATIONS AND SCIENTIFIC INVENTIONS,
IN RELATION TO
MEDICINE, SURGERY, AND HYGIENE.

WE propose noticing from time to time, under the above head, new mechanical inventions relating to surgery, medicine, and the allied sciences, novel medicines, sanitary appliances, dietetical, pharmaceutical, and chemical preparations, and other novelties, likely to prove interesting to some or all of the readers of this journal. We shall feel much obliged to those who may forward to us specimens of these novelties, upon which we shall, after due scrutiny, pronounce a critical opinion.

WARREN'S SWEET ESSENCE OF RENNET.

The old-fashioned method of preparing "essence of rennet" consisted in first making an infusion of the stomach of the calf, and then adding to the infusion strong salt brine and some muriatic acid. No doubt the essence of rennet, prepared in this manner, had good keeping properties; but it possessed a most disagreeable flavour, and produced anything but a pleasant whey.

Warren's sweet essence of rennet is prepared without the use of either acids or salt. It is perfectly sweet; but, at the same time, it is not a saccharine solution of rennet, for, on adding yeast to it, we do not find that fermentation sets in. We do not, in all cases, think it necessary to expose "trade secrets," when they are of a *bona fide* and harmless character; therefore, although we know the nature of the menstrum which holds the rennet in Warren's preparation in solution, we do not describe it further than as a perfectly innocuous substance.

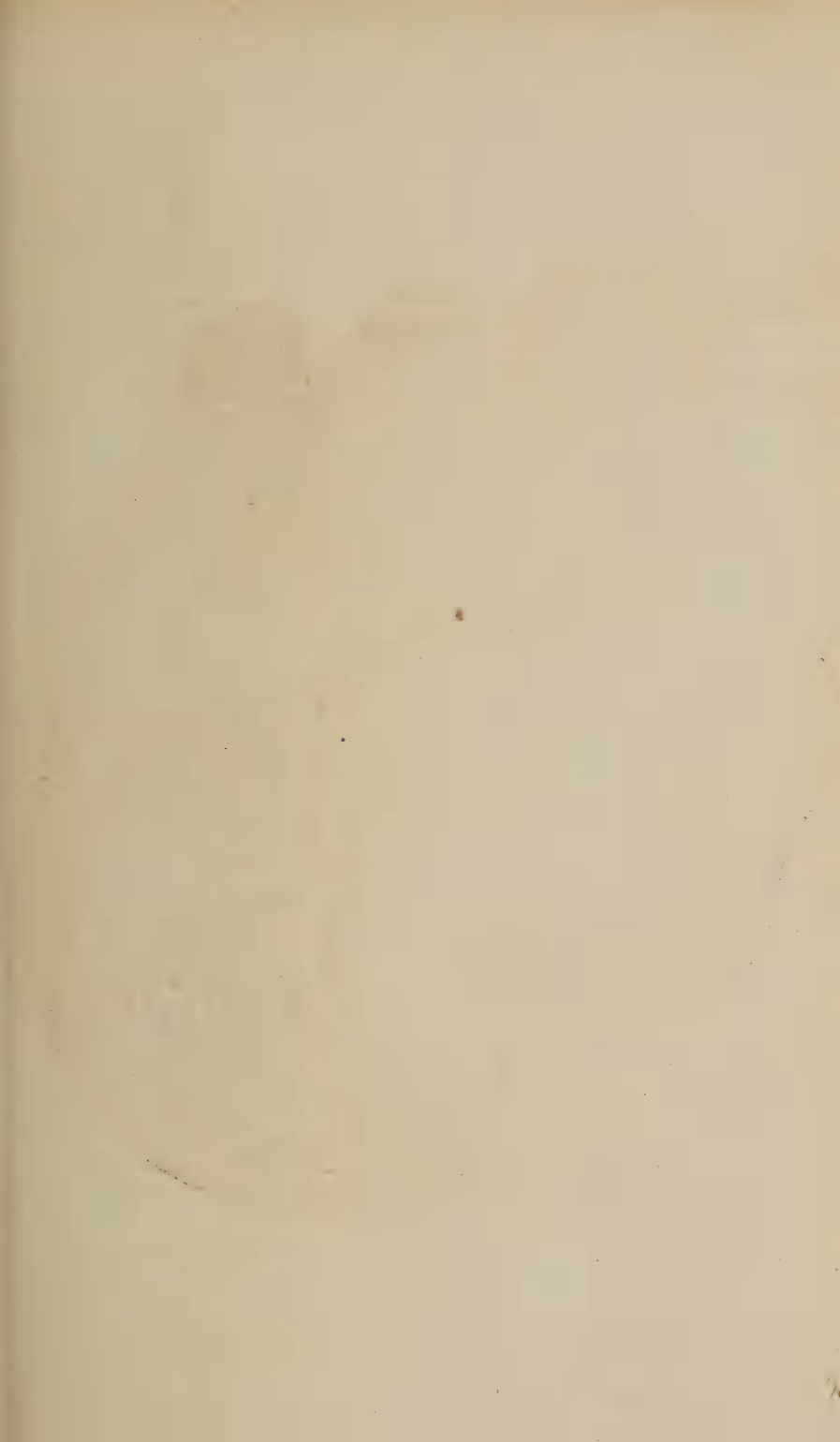
For many years both in hospital and in private practice we have been in the habit of using largely "junket," or rennetted milk, in febrile diseases; it is extremely refreshing to the patient, nutritious, and most easy of digestion, children, especially when they have sore throat, as in scarlatina, relish it when other food is refused, but it is most important that it should be prepared by means of a pure essence, and such we know Warren's to be.

NEAVES' FARINACEOUS FOOD.

Many of the farinaceous foods which are largely consumed as papulum for infants are composed exclusively of starch, derived from maize or other grain. Starch, at the most, can only form fat; it cannot produce blood, lean flesh, or bone. In the case of very young children, the digestion of starch is very difficult, owing to the absence of ptyalin from their saliva. Messrs. Neaves and Co., Fordingbridge, Salisbury, have introduced a food for children, which, unlike the starchy foods so much used for that purpose, contains all the constituents of a perfect aliment. It has been analysed by Dr. C. A. Cameron, who reports its composition to be as follows:

100 parts contain—					
Moisture	-	-	-	-	6.02
Albuminoids	-	-	-	-	17.40
Fats	-	-	-	-	2.24
Starch, &c.,	-	-	-	-	71.64
Cellulose	-	-	-	-	1.41
Ash	-	-	-	-	1.29
					<hr/> 100.00

The cellulose consists of bran reduced almost to an impalpable powder; and which will help to prevent the food from being so "binding" as the ordinary so-called farinaceous foods for children are said to be.





THE DUBLIN JOURNAL

OF

MEDICAL SCIENCE.

APRIL 1, 1872.

PART I.

ORIGINAL COMMUNICATIONS.

ART. IX.—*Cystic Tumour of the Lower Jaw.* By E. HAMILTON, M.D., F.R.C.S.I., one of the Surgeons to Dr. Steevens' Hospital.

P. M——, aged 40, by occupation a farmer, was admitted to Steevens' hospital, presenting a most remarkable and unsightly deformity, caused by the projection of the left side of the face. This was found to be dependent on a large tumour, which involved the inferior maxillary bone, extending from the posterior bi-cuspid tooth of the right side across the median line to nearly the level of the left tempero-maxillary articulation. The outline of the growth was irregular and nodulated, and at certain points distinctly conveyed the sensation of fluctuation. Along its upper border the depressed outline of the alveolar arch, containing the debris of carious teeth, could be recognized. Large veins ramified over the cutaneous surface, and an indented scar indicated where one of the cysts had opened and discharged a portion of its contents. The deep surface extended far back into the mouth, to the tonsil of the left side, which was considerably displaced by it. Careful examination proved that the motions of the tempero-maxillary articulation were still preserved, and that the neck and condyle were healthy. The

patient's aspect indicated fair general health. Physical examination elicited no evidence of visceral disease. There was no trace of glandular contamination. The patient gives the following history of his case:—

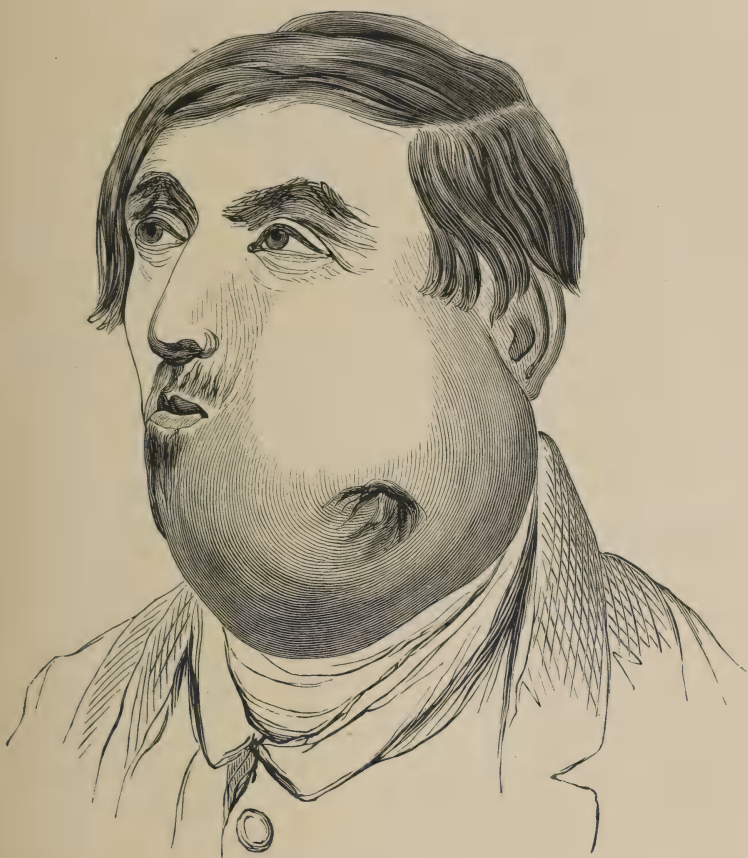
About three years ago he had three teeth extracted from the left side of the lower jaw. Their removal was followed by very smart bleeding, and from this date he observed the jaw begin to show signs of enlargement. The growth was at first slow, but within the last six months has been most rapid, and accompanied with intermittent lancinating pains. In March last the point indicated by the scar ulcerated, and discharged a quantity of clear fluid, tinged with blood. After about five weeks this opening healed, and has not since given him any trouble.

The case having been carefully examined by my colleagues, it was decided, in consultation, that it was one well suited for operative interference, which the increased rapidity of its growth imperatively demanded.

On the day previous to that fixed for the operation, the patient's bowels were well cleared out by a cathartic draught. At an early hour of the morning of operation a nutritive breakfast was administered, and before the inhalation of chloroform was commenced, he had an egg beaten up with brandy.

The patient was placed on a table, so arranged that one half of its leaf, by moving on a hinge, could be elevated to any desired height, and supported in that position. Full anaesthesia having been procured, the head and shoulders were elevated to the semi-erect position. The incision adopted was a single line along the base and ramus of the jaw, without any vertical division of the lower lip. The posterior bi-cuspid tooth of the right side having been extracted, an incision was made along the corresponding part of the base of the jaw, sufficiently large to admit the introduction of a metacarpal saw, with which the bone was deeply notched. The division having been completed with the curved forceps, the line of incision was extended upwards to the temporo-maxillary joint. The integument being very thin over this portion of the tumour, its division was much facilitated by the use of the broad flat director, the muscular attachments of the bone having been divided, and disarticulation effected, the diseased mass was easily turned out.

As the situation and nature of the wound precluded the possibility of carrying out the antiseptic treatment, and as the parts



MR. HAMILTON'S CASE OF CYSTIC TUMOUR OF LOWER JAW.

BEFORE OPERATION.



MR. HAMILTON'S CASE OF CYSTIC TUMOUR OF LOWER JAW.

AFTER OPERATION.

were very vascular, blood flowing from a number of vessels, it was considered advisable to depart from the plan of securing the vessels almost universally adopted at Steevens' hospital for the past two years—torsion, and to tie each vessel with a carbolized ligature, and cut the ends short. The flaps of integument were merely laid in apposition, but not secured, in order to anticipate the risk of hæmorrhage when re-action should be established, a precaution which was found to be quite necessary, as considerable bleeding took place before I had left the hospital. The flap was raised, and a large vessel secured; brandy and opium were given in small quantities and at repeated intervals until re-action was established, and sleep procured. The patient was fed three times each day with the œsophageal tube. No untoward symptom checked his convalescence. A small portion of the wound, from inversion of the skin, failed to unite; but the edges being vivified afresh, readily accepted immediate union. The mass presented the usual appearance of fibro-cystic growth so common in this region.

A plate was constructed by Mr. Baker, to prevent the falling in of the remaining segment of the jaw.

ART. X.—*Ununited Fracture of the Tibia and Fibula, treated by Re-section of Portions of the Tibia.* By EDWARD H. BENNETT, M.D., F.R.C.S.I., University Anatomist, Surgeon to Sir P. Dun's Hospital.

ON 19th April, 1871, a man named John Dwyer was admitted to the surgical wards of Sir P. Dun's Hospital, under my care. He was 34 years of age, unmarried, and had been all his life healthy until the occurrence of the injury from the effects of which he was then suffering. He was over six feet in height, good looking, and except that he was thin and worn by protracted suffering, healthy. I could discover no evidence of the existence of any of the diseases usually looked for in cases of ununited fracture—neither syphilis, scurvy, nor any other. He was a farmer, and had lived constantly in the country, in the county of Kilkenny. Nothing in his general condition seemed to be unfavourable to his recovery from a severe surgical operation, except the weakness induced by his long confinement to bed and his want of pluck. He was ready to weep and roar on the slightest provocation. However this excessive

sensitiveness, although a great annoyance in the protracted treatment of the case, did not appear to modify its progress unfavourably except on one most important occasion, namely, during the administration of chloroform while the operation of re-section was being performed.

In the month of December previous to his admission a cart laden with coal passed over his right leg and broke it. A surgeon set the limb, but it seems certain that a rough journey home and subsequent bad treatment caused a fresh displacement of the fracture. He stated that a wound of small size existed near the seat of fracture, which bled at the time of the accident, and from which small grains of bone came away during the suppuration which followed. On his arrival at home a bone-setter was called in and put the limb in splints. Ten days afterwards the bone-setter again visited him, and opened the bandages; he dressed the wound, and provided for the escape of matter which had accumulated under the first dressing. He promised to return on the twenty-first day and remove all splints. He kept his appointment, and on the day fixed removed all apparatus. At this time the leg was said to be straight and the fracture in good position. About a week after this a friend was in the house who had taken more to drink than was good for him; staggering, in consequence, he fell across the injured limb of his host. For some time after this the broken limb was very painful, and four small grains of bone came away in the discharge from the wound, which was poulticed, but not supported by any splints. About a week after the infliction of this second injury on the limb a small lump began to appear on its inside, which the bone-setter considered to be proud flesh, and promised to cut off if it increased. Day by day it grew until it became evident that it was formed by the protrusion of the upper part of the fractured bone.

On admission to hospital, four months after the accident, the limb presented the appearance seen in the accompanying woodcut, No. 1, taken from a plaster cast. The lower end of the upper fragment of the tibia projected on the inside, and was covered by thin adherent skin, which was in parts ulcerated, in others red and glossy. The lower fragments of the bones formed an angle with the upper fragment of the tibia, so that the heel was displaced backwards and outwards. The foot was inverted to such a degree that as the man lay on his back in bed it rested on its inner border; the upper end of the lower fragment of the tibia could be felt and

seen projecting against the skin, outside the upper fragment, and nearly three inches above its lower extremity. This projection is distinct in the cast, and appears in the wood-cut. It was not possible to determine the condition of the fibula with the same certainty as that of the tibia, for the tissues covering it were thickened and œdematous, so as to prevent the finger from following its outline. I have but little doubt, however, that it had been broken in two places—first, near the level of the fracture of the tibia, and again about three inches higher up. On grasping the foot, it was evident that the fractures were ununited; the lower segment of the limb could be moved with considerable freedom in any direction, the motion of rotation being most free. The wound which had occurred at the time of the accident was healed, and a slight mark showed its place in the depression between the overlapping fragments of the tibia, by the lower of which it had most likely been formed. The limb was two and a-half inches shorter than its fellow, its posterior aspect was flattened, and its tissues, particularly in the lower part, were œdematous.

The examination of the limb showed clearly that the fracture was an ordinary oblique fracture of the tibia, the line of fracture running from above and behind, downwards, forwards, and inwards, so as to place the point of the upper fragment on the subcutaneous surface of the tibia. A sufficient time had elapsed to allow of the rounding of the sharp point of the upper fragment by absorption, a change chiefly effected by its pressure against the skin; at least, such would appear to be the correct explanation of the difference between it and the similar point of the lower fragment, which (as the wood-cut No. 4 shows), retains its original sharp and angular outline; this had lain bedded in the tissues of the limb unexposed to any pressure, while the other had the strained integument constantly drawn tightly over it.

The nature of the injury being clearly made out, the questions next in importance were—what cause could be assigned for the non-union of the fracture? and what was to be done to render the limb useful, or at all events, to relieve the patient from his condition of confinement to bed.

There existed no constitutional disease that I could discover to explain the non-union, so I was forced to look for a local cause. The fracture, though oblique, was nothing extraordinary, and although great displacement had been allowed to occur, still the fragments appeared in apposition, and they had been sufficiently at

rest, the man having been in bed with some kind of support applied to the limb for the greater part of four months. The two conditions essential to the union of fracture, rest and apposition, had apparently been satisfied.

The idea that the failure of union was the result of osteitis suggested by some who saw the case seemed to be set aside by the fact that the wound which had communicated with the fracture during the period of suppuration had healed after the separation of a few minute particles of bone, and by its healing showed that no further disease of bone existed.

But two modes of dealing with the case deserved consideration—namely, amputation, or an attempt to remove, to some extent, the deformity, and induce union by re-secting such an amount of bone from the end of either fragment as would admit of their reduction, and of the rectification of the limb, and by its performance stimulate the union of the bone.

My colleagues agreed with me in the propriety of resorting to the latter method.

On April 27th, I operated, assisted by Drs. Butcher and Little.

I made an oblique incision from the extremity of one fragment to that of the other, the wound of which is still visible in Fig. No. 2, and divided all the structures down to the bone. I then raised the periosteum with a blunt elevator from both fragments. As I attempted to do this completely, I was interrupted by a mass of tissue lying in the interval between the fragments in front; this I raised out of the space, and removed. I then projected the lower fragment forwards, which was done to some small extent with facility; but, beyond this I feared to go, as the force necessary appeared to me to be too great. I passed a chain-saw behind the fragment, but broke it in the attempt to work it. I then cut off the fragment (Fig. No. 4) with a powerful bone forceps, and then cut with the same instrument an even surface on the upper end of the fragment. This being done, it was easy to apply Butcher's saw to the extremity of the superior fragment, and after three transverse sections of the bone it was possible to reduce the fragments to a straight line. On the posterior part of the fragments, some pieces of new bone were formed in the periosteum and surrounding tissues, but not sufficient to support the fracture and prevent motion. Before reduction was possible it was necessary to apply considerable force in order to detach the fibula from its faulty connexions,

Fig. 1.



Fig. 2.

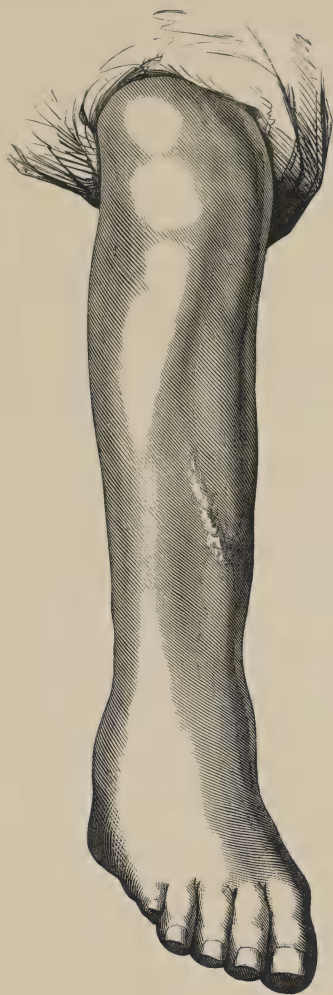
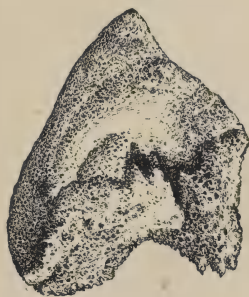


Fig. 3.



Fig. 4.



but in this way I succeeded without being obliged to expose the bone or cut it. The operation, necessarily a tedious one, was unfortunately much hurried by the bad behaviour of the patient under chloroform. At one time, just as I was clearing the first piece of bone and applying the chain-saw, he showed marked symptoms of an inclination to faint, and caused me much anxiety; so that I was forced to suspend the chloroform in great measure, and make the best haste I could to finish my work. To this I attributed my breaking the chain-saw, for it was slow to start its cutting on the rough posterior surface of the bone, and was broken by too hasty action. I had been prepared to use Dieffenbach's pegs, in order to stimulate the union of the fracture, but for two reasons I laid them aside. First—I considered that I had found a sufficient cause for the non-union of the fracture in the mass of tissue which I found interposed between the parts of the fractured bone. This mass of tissue was, as far as I could judge during the haste of operation, composed of muscle and tendon. Secondly—I considered the condition of the patient, resulting from the effects of chloroform, too dangerous to allow of any prolongation of the time of operation, beyond what was absolutely necessary to effect the reduction of the fracture.

No ligature was used during the operation, and but slight oozing of blood occurred after re-action set in. For more than an hour after the patient was removed to bed, the lower segment of the limb caused me great anxiety, for it was cold, livid, and its vessels were pulseless, but gradually it recovered its heat and colour, and the arteries could be felt beating. The limb was put up in a box-splint, with the knee slightly bent and raised; except for trouble, caused by sloughing of the heel, and of the skin over the crest of the tibia caused by the pressure required to retain the fragments in the straight position, the case progressed steadily, though very slowly, to recovery.

The patient was discharged from hospital on January 6th, 1872, and returned to Kilkenny, able to get about with a crutch and stick, the bone firmly united, and nothing but time and use required to establish the full action of the limb; the shortening was barely $2\frac{1}{2}$ inches, rather less than that of the limb before operation.

Fig. No. 2 represents the appearance of the limb at the time of the patient's leaving hospital. It is of necessity shortened, and not a very pretty limb to look at, but it is straight, firm, and well able to bear weight. I believe much of the loss of power is caused by

the damage done to the anterior muscles by the fracture and subsequent operation, particularly to that portion chiefly of the extensor pollicis, which was cut out from between the fragments.

The wound of operation was still in part uncicatrized when the patient left, but it has healed much since, and will, I have no doubt, close as soon as the limb acquires a firmer and more healthy condition by use and residence in the country.

The consideration of the facts of the case before, during, and after operation point clearly to the conclusion that the cause of non-union of the fracture was the interposition of a mass of muscular and tendinous tissue between the fragments, and not either the obliquity or want of rest of the fragments, nor again, any diseased condition. The fact of union having been attempted posteriorly, as the callus deposited in this part showed, is strong evidence in support of this conclusion.

ART. XI. — *Case of Elephantiasis Græcorum (Leprosy).* By
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THERE are many considerations which render the study of the disease Leprosy (or according to modern nomenclature, Elephantiasis Græcorum) an attractive one. Whether we regard it as possessing a longer line of ancestry than perhaps any other existing disease of like severity, dating more than three thousand years back, or its prominent position in the sacred writings, the horror and detestation in which its victims were held from earliest times, and even yet are held in many countries where the disease is still endemic, or the inevitable destitution and mendicancy that attend its existence among a population; in whatever aspect we consider it, the interest is not lessened, nor the duties of investigating its phenomena diminished. Still less, when we find that the evidence is all but conclusive that the disease very rarely, if ever, manifests any tendency to a spontaneous cure, and that there is no medicinal substance, mineral or vegetable, which has yet been found to exert anything like a direct or specific effect on the malady. Nor was its pathology a matter of less obscurity than other relations of the disease, until Virchow developed the

comparatively crude observations of Danielssen and Boeck and others, and placed it on a basis of scientific truth.

Good service has been rendered to the clinical history of the disease by the "Report on Leprosy, by the Royal College of Physicians" of London in 1867, and much has been done by the report to indicate the probable causes that favour its development and persistence on the one hand, and its disappearance from whole tracts of country on the other. Though up to a few centuries ago the disease was prevalent in the British Isles, it may now be considered one of the very rarest of indigenous diseases in these islands: most, if not all, cases of the disease now occurring in these countries being imported from foreign parts, where it is still endemic.

The following are brief notes of the case of a man suffering from the disease in question, who was sent me by my friend Dr. Stirling of Thomastown:—

T. H., aged forty-seven, a native of the Co. Kilkenny, enlisted in the service of the East India Company, and left Ireland for India in the year 1844. While at home, previous to his departure, he had always enjoyed good general health. He denies having ever had syphilis. In Aden he had an attack of liver disease, for which he was in hospital eighty days. In Madras he suffered an attack of dysentery, and another of ague. In Borneo he got a second attack of liver disease; and in Bengal, during the mutiny, he had fever.

After a protracted service in India of twenty-seven years, he returned to these islands about two years and three months ago, and enjoyed excellent health for about a year and a-half. But about nine months ago he began to be conscious that his health was not up to its accustomed standard, and soon he suffered a feverish attack, followed by sensations which he described as being those of creeping, itching, and tingling all over the body. The first morbid growth that attracted the man's attention was on the backs of the hands and the fingers, which became swollen, tense, and itchy, resembling chilblains. He soon became aware that the disease appeared also on the face, head, neck, and shoulders, and assumed a tuberculous character.

When the patient was admitted into the City of Dublin Hospital he was found to have lost flesh to a considerable degree. The general surface of the body presented a dusky, brownish, or

reddish-brown appearance. Scattered in great numbers over every part of the body, from the crown of the head to the soles of the feet, were spots of a darker hue, with a somewhat coppery tinge, suggesting at first sight a connexion with syphilis. The spots averaged in size about that of a fourpenny silver piece. Some of the spots faded slightly in colour under pressure, and the skin underneath felt perfectly normal to the touch. Others preserved their hue unchanged by any amount of pressure, and the skin underneath felt decidedly indurated, though not elevated above the general level of the surface, while a third set had not only undergone pigmentation and induration, but were elevated in varying degrees from a very slight rise, like that of urticaria, up to the condition of a prominent rounded sessile tubercle. But this tuberculous condition was not equally distributed over the body. It was very much more marked on the face, neck, forearms, and hands than elsewhere; and in an intermediate degree on the shoulders and arms. On the wrists, hands, and back of neck the tubercles had reached their largest. Here a few had attained to the size of a large bean. The eyebrows, from their tuberculous condition, presented a heavy, overhanging effect. The eyelids owing to the same, appeared greatly thickened, bloated, and half closed, causing a most sleepy lethargic expression of countenance. There was a remarkable deficiency of eyelashes. A few very small tubercles were found on the palpebral conjunctivæ—none on the ocular, but the latter were vascular, and the eyes tender and watery. A very scanty growth of hair on the face was observable. The face presented a remarkable aspect. It was of a dusky, reddish-brown colour, and had a swollen, puffy, bloated appearance, studded thickly with sessile tuberculous masses in all stages of development, varying from the size of a No. 3 shot to that of a filbert nut. Near the angles of the mouth two or three tubercles appeared more advanced than elsewhere. They were about the size of a filbert nut, very prominent, tense, and elastic, smooth, shining, of a semi-transparent, porcelainous whiteness, with a few vessels ramifying over their surface. The entire of the mucous membrane of the lips, cheeks, palate, pharynx, and under surface of tongue were thickly studded with small tubercles, and in the latter situation some had ulcerated, and there was a certain amount of swelling and inflammation of the organ, causing a very imperfect articulation. The mucous membrane of the nose was likewise affected, giving the voice a nasal tone. By the aid of the

laryngoscope it could be seen that the disease extended as far as the commencement of the œsophagus, and the mucous membrane of the arytenoid cartilages and the ary-epiglottidean folds were closely studded with tubercles, some having attained the size of a small pea. Partly from this condition of the mucous membrane, and partly from the irritability of the throat, inducing reflex spasms at every introduction of the laryngeal mirror, and partly, also, from the condition of the tongue, precluding the possibility of grasping it with sufficient firmness, the vocal cords could not be brought into view. However, from the peculiar hoarseness of the voice,^a there can be little doubt that the disease extended to the cords. There was no evidence that the disease had spread further into the gastro-pulmonary mucous tract. There was no tendency to diarrhœa. No fœtor of the breath. The urine contained no albumen; its sp. gr. was 10·20.

A careful examination of the surface of the body failed to reveal the presence of anæsthesia anywhere.

The treatment adopted in this case may be dismissed in a very few words; at first, iodide of potassium, and afterwards arsenic and cod liver oil, along with simple warm baths. I need hardly say no perceptible effect was produced in three weeks, the length of his sojourn in hospital.

ART. XII.—*Notes on Operative and Conservative Surgery.* By AUSTIN MELDON, Licentiate of the King and Queen's College of Physicians and of the Royal College of Surgeons, Ireland; Surgeon to Jervis-street Hospital, and late Demonstrator of Anatomy in the Catholic University of Ireland.

- I. DOUBLE DEPRESSED FRACTURE OF THE SKULL—TREPHINING—HERNEA CEREBRI.
- II. GUNSHOT WOUND OF THE ARM—GANGRENE—AMPUTATION AT THE SHOULDER-JOINT
- III. GUNSHOT WOUND OF THE ABDOMEN.
- IV. FRACTURE OF ALL THE BONES OF THE FACE.
- V. A FEW CASES OF AMPUTATION—PRIMARY AMPUTATIONS OF SHOULDER AND ARM.

^a Dr. James Little, to whom I had the pleasure of showing this patient, said that, having been in the East, in parts where leprosy was not very uncommon, he recognized this man's voice as being most characteristic of the disease.

- VI. ELEPHANTIASIS OF THE LEG—CIRCULAR AMPUTATION OF THIGH.
- VII. AMPUTATION OF THE THIGH BY DOUBLE FLAP OPERATION.
- VIII. LARGE ULCERATED SCIRRHUS TUMOUR OF THE BREAST—AMPUTATION—RECOVERY.
- IX. RAPID GROWTH OF A FATTY TUMOUR OF THE BREAST—REMOVAL.
- X. EPITHELIOMA OF THE HAND—AMPUTATION OF THE FORE-ARM.
- XI. TWO CASES OF AMPUTATION OF THE FOREARM, THE RESULT OF HUMAN BITES.
- XII. STRANGULATED INGUINAL HERNIA—ARTIFICIAL ARMS.
- XIII. A T-SHAPE FRACTURE OF THE FEMUR EXTENDING INTO THE KNEE-JOINT.
- XIV. RUPTURE OF THE URETHRA—EXTRAVASATION OF URINE.

I TRUST that the following notes of operative and conservative surgery may be found by my professional brethren worthy of a perusal. The first case speaks for itself. A case of recovery after a double operation of trephining, where the operation has been performed before symptoms of compression set in, carries with itself sufficient interest. The next two cases are gunshot wounds. In one I tried to arrest spreading gangrene by amputation at the shoulder-joint. In the other the bullet entered the abdomen, producing no other bad symptoms than retention of urine for several days. The case of fracture of all the bones of the face shows what nature and conservative surgery acting in harmonious combination are capable of doing. In the case of elephantiasis, although the affection presented all the appearances of true Arabian elephantiasis, it was preceded if not caused by acute inflammation of the lymphatics. My senior colleague, Dr. Stapleton, proposed to call the affection Chronic Angeio-leucitis. In removing the extremity I performed the circular operation, and for sake of comparison I give the short notes of a case in which I performed the double flap. I believe that in every case where there is much exhaustion the circular is a far better operation than any of the various flaps which have been recommended. I record a case of amputation of the breast where the scirrhus had already ulcerated, and where more than one gland in the axilla had become engaged; notwithstanding which, the patient recovered, and has not since had any symptom of a return of the disease. I conclude

this report by three cases, which I hope may be found to possess the points of interest which induces me to add them to this paper.

CASE I.—*Double Depressed Fracture of the Skull—Operation—Hernia Cerebri—Recovery.*

James Foint, a healthy-looking boy of seven years, was brought to Jervis-street Hospital on the 16th of August, 1868. Two hours previously he had been found in a field, insensible. On admission he was semi-conscious, the extremities cold, the pulse 120 and compressible, the respiration natural; one pupil was contracted, the other dilated. Two large lacerated wounds on the scalp were visible: one situated near the parietal, the other over the occipital bones. These wounds were of a crescentic shape, rendering almost certain the supposition of those who found the child, that the injuries were caused by a horse let loose in the field. From either of these wounds the finger passed through the bone a considerable distance into the interior of the skull; the depression being at least an inch in depth. It was evident that even a child's brain could not bear this pressure. In consultation with my colleagues it was agreed that an early operation would give the patient the best chance of recovery. Everything necessary being ready, the child was placed on the operation table, and the wound over the parietal bone enlarged to a sufficient extent to enable the necessary instruments being used. With the assistance of Hay's saw and a trephine an elevator was got under the depressed portion. It was necessary to use great force in order to elevate it. The dura mater was found lacerated, and several portions of loose bones had to be removed. An incision was next made across the second wound—that over the occipital; the flaps dissected back, and in the same manner the bones raised. Here the state of the dura mater was not seen. A large piece of bone being entirely loose when elevated, was removed, the wound lightly dressed, the patient brought back to bed, and put on small doses of hydrarg-cum-cretâ. On the following morning the pulse was 120 and intermittent, the skin moist, the pupils dilated, and the patient had slept a good deal. On the second night after the operation the child was restless. On the 19th of August he had distinct rigors, and complained of much pain in the head. On the 20th the condition of the patient was worse: the pulse was 124, hard, and intermittent, the skin dry, the bowels constipated, and the child was exceedingly restless. Under suitable treatment,

however, these symptoms subsided, his state improved, and on the 25th instant he had made such progress that great hopes of his recovery were entertained. At the morning visit on the 28th a small hernia cerebri was first perceived; a slight compress of lint was placed over it, but in two days it had become as large as a pigeon's egg. Considerable compression was then employed by means of lint and tea lead. At first this was thought to increase the hernia, but subsequent observation proved such was not the case. A good deal of discharge now took place, and it became evident that the tumour was sloughing away. Three days later the discharge had lost all offensive smell, and the tumour looked much smaller; it was soft and clean, and presented somewhat the appearance of "scarlet velvet." A fortnight later it had entirely disappeared, the wound gradually closing over it. The boy grew stout, and left the hospital on the 6th of January, 1869, perfectly well. Even now, after the lapse of three years, the brain can be seen pulsating under the scalp in both the places where the fractures existed.

There are, I feel assured, many cases of compression of the brain which might be saved by early operation. In waiting for symptoms we too often lose valuable time, inflammation often setting in before we think of raising the depressed bones. In this case I am convinced that the recovery was due to the early operation. In applying compression over the hernia cerebri I used the ordinary tea lead, laying one fold over the other. Too much pressure produced spasmodic closure of the eyelids and convulsive movements of the arms and legs. On removing a single layer of lead these ceased. Four folds of it were used throughout.

CASE II.—*Gunshot Wound of the Forearm—Gangrene—Amputation of the Arm at the Shoulder-Joint.*

James Waters, a strong muscular man, aged thirty-six years, a blacksmith by trade, was admitted into hospital on the evening of the 15th of September, suffering from gangrene of the right arm, extending to within four inches of the shoulder-joint.

On Wednesday the 11th, he had received a gunshot wound in the fore-arm, below and in the immediate proximity of the internal condyle of the humerus. The shot did not lodge, but appears to have carried away the integuments, muscles, and blood vessels, leaving the radius bare, but not fracturing either bone. The hæmorrhage was immediately checked, and the wound dressed.

On Friday the 13th, two days after the accident, gangrene set in. The surgeon who attended proposed amputation, but the patient would not consent to the operation.

The gangrene continued to advance until Sunday, the day of admission into the hospital, and was well marked all up the hand and arm. He had not slept since the accident. A charcoal poultice was applied, an opiate given, and stimulants freely administered.

On Monday 16th, I amputated the limb at the shoulder-joint. The operation consisted in making a triangular flap from the deltoid by transfixion, disarticulating and then cutting the under flap. There was a good deal of hæmorrhage from the circumflex arteries, which had to be controlled before the operation was completed. The upper flap was a good deal discoloured; the under, although apparently healthy was necessarily cut small, as the parts in the vicinity were œdematous. He slept well after the operation, was free from pain, and seemed in good spirits. During the night he sweated profusely, and complained of great thirst. Brandy and beef-tea were freely administered.

17th.—Patient easy; complained of no pain; stump warm. Pulse 104, weak and compressible; skin acting very freely; sero-purulent discharge from the wound; ordered turpentine punch. 6 p.m.—Temperature of flaps continued normal. 10 p.m.—Temperature of external flap had fallen several degrees. During the night bullæ formed, and its surface assumed all the characters of gangrene, the internal flap remaining healthy.

18th.—The patient seemed worse; slept badly; pulse very weak and compressible. The gangrene extended to the neck and anterior part of the chest.

19th.—Patient passed a bad night; pulse flickering and barely perceptible; right shoulder and neck much swollen; complained of difficulty of breathing; accumulation of mucous in the mouth and larynx. Patient sunk gradually until 1 p.m., when he died.

A *post-mortem* examination showed that the axillary artery had become obliterated, being completely blocked up by lymph. No other pathological lesion of special importance was discovered.

CASE III.—*Case of Gunshot of Abdomen.*

On Friday, November 16th, William Farrell, aged thirteen years, was admitted into hospital suffering from a gunshot wound of the abdomen, which had been accidentally inflicted by a revolver bullet. The ball (a conical one) struck the cartilage of the eighth

rib on the left side, about two inches from the ensiform cartilage and three inches from the umbilicus. On admission the boy was in a state of collapse. There was but slight hæmorrhage; the pulse was 54; the wound was found to be penetrating; the patient was ordered large doses of opium. Three hours after the abdomen was extremely painful; relief was obtained by stuping; the pulse had increased to 70. For six days after admission the patient suffered from retention of urine, but felt in other respects quite well. On the 7th day he recovered power over the bladder. On November 29th, the patient was ordered a demulcent enema which operated well. He left the hospital, December 21st, quite well. There was throughout no indication of the position of the ball.

CASE IV.—*Severe Fracture of all the bones of the Face—Recovery.*

Thomas Dillon, aged thirty-five years, was conveyed to the hospital on the 3rd September, 1870. As a labourer he was employed raising heavy weights on board vessels by means of a windlass; whilst so occupied the handle slipped, and flying round struck him on the face with tremendous force. On admission into the hospital he was quite insensible, and the face presented a frightful appearance. He had evidently been struck from the angle of the mouth on the right, to the edge of the orbit on the left side. Both upper maxillary bones were fractured. The nasal bones pulverized; the prominent portion of the orbit flattened; the palate comminuted, and all the upper teeth, with their alveolar processes, and portions of the upper maxillary bones hung loosely in the wound. The inferior maxillary bone was also fractured. The large flap containing part of the nose and upper lip were turned down over the chin and neck, suspended by two narrow portions of skin and muscle. A good deal of venous oozing was checked by cold. The upper teeth were replaced, and the soft parts brought as nearly as possible together, and kept in their places by means of sutures, together with an ordinary mask and a bandage. The right eye was found much chemosed, and although the lid was divided it escaped all further injury. When the patient recovered sensibility it was found he could not swallow, but on the following day when the palate was fixed by means of a gutta-percha plate, power of swallowing returned. The patient made a rapid recovery, and left the hospital with a very good face. The teeth and bone attached remained for a considerable time so loose that they could easily have been removed by the finger and

thumb; they have now, however, become quite firm, and he has made an excellent recovery. Some months after his leaving the hospital, I performed a successful operation for entropion, caused by contraction after the accident; of the entropion there now remains not a trace to be seen.

A FEW CASES OF AMPUTATION.

CASE V.—*Primary Amputation of the Shoulder.*

John Hands, was admitted into hospital, on 21st of August, 1868. The hand and arm had been very much crushed three hours before admission, in a threshing machine. During his journey from Swords he had lost a considerable quantity of blood. Immediately on admission I removed the arm from the shoulder; he soon rallied after the operation, and left the hospital on the 4th November, perfectly well; no bad after consequences have since presented themselves.

Primary Amputation of the Arm.

Jane Sands, aged ninety years, was brought to hospital, on 18th October, 1867, with a bad compound comminuted fracture of the arm. On admission she was moribund from loss of blood and fright. Considering her too weak for the operation, digital pressure on the brachial (which had been divided), was kept up during the night. Early on the following morning I removed the arm by the circular operation, the bone being sawn through immediately below the joint, little more than the head being left on the socket. By the 1st of November the wound had completely healed, and were it not that the ligature remained, she would have been discharged. She left the hospital on the 22nd of November, quite recovered.

CASE VI.—*Elephantiasis of Leg—Amputation of Thigh.*

Henry Campbell, a boy of seventeen years of age, was admitted into hospital, on December 8th, 1869, suffering from elephantiasis of the leg. Three years before he first complained of a dull pain on the inner side of his ankle. This first symptom he attributed to being, from his occupation, constantly with wet feet. This sensation soon spread to his entire foot, all the pain being referred by him to the skin itself. Various applications were now resorted to to procure relief, and to these he attributed the swelling which then appeared. Gradually this swelling spread upwards, always

preceded and accompanied by pain of a dull weighty character; slowly at first, more rapidly of late, did the limb enlarge until the time of his admission, when it measured $17\frac{3}{4}$ inches round the instep. It presented all the appearance of true elephantiasis. The limb was heavy and cumbersome; hard, dense, unyielding and rough; darker in colour than natural, fissured, nodulated, and completely deprived of all its normal sensibility. Twelve months previous to his applying at the hospital, an abscess formed along the course of the lymphatics; since then several had formed and burst. The pain, together with the constant discharges, had reduced him to the lowest possible state of exhaustion. He was for a considerable time in the hospital before he was considered strong enough to bear an operation. On the 16th of April, 1870, I amputated the limb by the circular operation. He made a good recovery, and left the hospital with an excellent stump.

CASE VII.—*Double Flap Operation.*

As a comparison I select a stump formed by the double flap operation. The history of this case is as follows:—

Michael Lalor was admitted into Jervis-street hospital, on 25th October, 1867. For three years he had been unable to do anything, and was now in the height of hectic fever. The knee-joint had been in a state of suppuration during that time, and on his admission there was considerable disease of the bones. At his earnest entreaty I removed the limb by a double flap operation, dividing the bone at the junction of the lower and middle third of the femur. There were upwards of thirty vessels requiring ligature, three of these were as large as an ordinary femoral. The flaps were left to glaze and in a single week were entirely united by first intention.

CASE VIII.—*Large Ulcerated Scirrhus Tumour of the Breast—Removal—Recovery.*

Jane M'Fallon, aged thirty-eight years, came to the hospital on the 25th of April, 1870. Two years before she first suffered from the lancinating pain, and first noticed the small stony hard kernel so characteristic of scirrhus. On admission the left breast was the seat of a very large tumour, which had ulcerated some three months before, and had bled on several occasions. Some glands in the axilla were found enlarged. At her earnest request I removed the breast a week after her admission; two glands were dissected out.

So large was the tumour and so much of the skin had to be removed that it was found impossible to make the edges of the wound meet. A large gap was therefore left to heal from the bottom. She returned home on the 9th of June with the wound almost healed. Up to this date, Christmas 1871, she has remained in health, no sign of the affection having returned.

CASE IX.—*Rapid Growth of a Fatty Tumour of the Breast—Removal.*

Catherine Hogan, a healthy-looking woman, of forty years of age, was admitted under my care on the 2nd September, 1869. One month previously she had first noticed a small tumour about the size of a nut in the right breast. On admission it had already reached the size of an orange, presenting all the appearance of a fatty tumour. She was immediately placed on the operation table, an incision made, and the tumour removed. The parts healed by first intention, and in a week's time she was discharged well.

CASE X.—*Case of Epithelioma of the Hand—Removal—Return—Amputation—Recovery.*

Ellen Byrne was admitted on the 8th April, 1869. Twelve months previously I had removed a large epithelioma from the back of the hand. The wound healing rapidly, she left the hospital, apparently cured. She now stated that she had remained free from the affection until six weeks ago. On re-admission the epithelioma was as large as it had been when she had been previously admitted. Being now adherent to the bone, I amputated at the forearm on the 10th of April, 1869. She left the hospital on the 25th of the same month, the stump being perfectly healed. Since then she has had no return of the disease, and appears to be quite cured.

CASE XI.—*Diffuse Inflammation, the Result of Human Bites.*

Within the last few years many cases of diffuse inflammation admitted in Jervis-street Hospital have been caused by human bites. From many such I select the short notes of the following cases:—

Daniel M'Kernan, an old man, much given to drink, was bitten by his son on the 14th November, 1870. Four days after, he applied to hospital with diffuse inflammation of the arm. Free incisions were at once made, and suitable treatment adopted. The

bones, however, became diseased, and a month later the arm was amputated.

Mooney, a man apparently beyond sixty years of age, applied at the hospital on the 24th day of February, 1870. His history was that a month previously he had been bitten. Diffuse inflammation resulted, and on admission the bones were found to be extensively diseased. His strength had been much exhausted by long continued suppuration. A short time after his admission I amputated the forearm. Immediately after the operation he recovered strength, and made a rapid recovery, and left the hospital on the 10th day of April, 1870.

CASE XII.—*Strangulated Hernia.*

Nicholas Stafford, fifty-four years of age, was admitted under my care on the 29th of August, 1871, with a very large strangulated inguinal hernia. He stated it had been down but a few hours. On admission it was of immense size, and very tense. Being unable to reduce it in the ordinary way, I determined to operate. Having made the usual incisions, I exposed the sac and freed the stricture. It was now found that the entire intestine which formed the hernia was gangrenous, and burst on the slightest touch. I formed an artificial anus with a piece of healthy intestine drawn down for the purpose, and put the patient to bed. On the third day he died of peritonitis. I record this case to show how little reliance can be placed on the statement of hernia patients applying at hospitals. In this case there is no doubt that the hernia had been down for a considerably longer time than stated, gangrene would not otherwise have been found to prevail so extensively.

CASE XIII.—*T-shaped Fracture of the lower third of the Femur.*

Joseph Kearns, aged seventeen, was admitted into Jervis-street Hospital in an insensible condition on the 21st of February, 1870. Whilst painting the mast of a vessel, the board on which he sat gave way, and he fell a distance of thirty feet. He was immediately picked up insensible, and conveyed to the hospital. On examination a large bloody tumour was found on the head; pulse quick and compressible, pupils dilated, breathing quiet; the left thigh fractured; fracture extending unto the knee-joint, the internal condyle being everted; could be felt under the skin. At first, the injury looked like dislocation of the tibia inwards, but on closer examination

it was found to be a T-shaped fracture of the lower part of the femur. For three days the patient lay insensible, supported by enemata. Great difficulty was experienced all through in preventing part of the condyle, which was very sharp, from coming through the skin. On the fourth day he became sensible, and progressed favourably until the 16th April, 1870, when he was discharged.

An immense quantity of calus had been thrown out on and about the internal condyle, forming a tumour about the size of a large turnip. This is yet of considerable size, but nature has done something towards lessening it.

He has been long since enabled to resume his occupation, and is now as active as ever. The joint is perfect in all its motions. All efforts at the time of his admission to reduce the deformity were in vain.

On the night of the 27th of October, a man was brought to the hospital. For nearly three days he had been unable to void his urine. An hour previous to my seeing him the urethra had given way, and the urine was infiltrated into the cellular tissue. Free incisions were at once made; a good deal of sloughing both of scrotum and penis took place, but ultimately the patient recovered with comparatively little deformity.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

A Treatise on Diseases of the Nervous System. By WILLIAM A. HAMMOND, M.D.; Professor of Diseases of the Mind and Nervous System and of Clinical Medicine in the Bellevue Hospital Medical College. New York: D. Appleton and Co. 1871. 8vo, pp. 754.

THE appearance of a systematic work, in the English language, on the subject of diseases of the nervous system, is an event of so unfrequent occurrence as to deserve to be regarded with unusual interest. A number of monographs, some of them of great value, on different affections of the nervous system, have been published, but we are not aware of any attempt having been made in these countries, of late years, to give a connected account of this extensive subject on an adequate scale. A large space has certainly been devoted to nervous diseases in the *System of Medicine*, edited by Dr. Russell Reynolds, but, in this work, the different parts of the subject are treated by different writers, the articles are of very unequal merit, and, in some important particulars, the pathological and therapeutical principles held by several of the authors, seem to be rather widely at variance. In the work before us, by an American author, already creditably known by his contributions to the study of nervous diseases, we have a tolerably complete account of this important subject, executed on a uniform plan and with great care. In dealing with an extensive subject of this kind, in which so much is still only imperfectly known, it is impossible that there should not be many points on which discrepancies of opinion will exist, and on some subjects we shall be obliged to express our dissent from the author's views, but we have no hesitation in expressing our conviction that Dr. Hammond's work will be found to contain an excellent summary of the present state of knowledge on the subject of nervous diseases, and that the practitioner can refer to its pages with confidence and advantage. The writings of Flint, Bumstead, Da Costa, and others, have taught students of medicine, in these countries, to regard the works of some of our transatlantic brethren as standard authorities on

various subjects. Dr. Hammond, we are glad to be able to say, has produced a treatise not unworthy to rank in the same list with those of the eminent authors whom we have named.

In his introductory chapter, the author gives an account of the instruments and apparatus employed in the diagnosis and treatment of diseases of the nervous system. Among these the author regards the dynamograph as an instrument of great diagnostic value. It consists in a dynamometer to which a clock-work arrangement is attached, similar to that employed in the sphygmograph of Marey. The patient grasps the dynamometer, the paper is set in motion, and, as it moves, a pencil traces upon it a line, the height and regularity of which line depend on the amount of firmness, and on the steadiness with which the patient exerts pressure on the instrument. The dynamograph, therefore, according to Dr. Hammond:—

“The dynamograph, therefore, writes down the muscular power and tone of the individual, and likewise indicates the perfection of what is sometimes called the muscular sense. A person in good health will make a straight line with the pencil. If there is paralysis of the muscles of the arm, or incoördination to the slightest possible extent, the line will be irregular. The papers used may be marked with the date and the name of the patient, and thus a record of his condition is preserved.”

After this we have five sections, the first treating of diseases of the brain, the second of diseases of the spinal cord, the third of cerebro-spinal diseases, the fourth of diseases of nerve-cells, and the fifth of diseases of the peripheral nerves.

Of the subjects of cerebral congestion and anæmia we could have desired a somewhat fuller account, as the question of the variations in quantity of the blood in the cranial cavity is one of great practical as well as pathological importance. On one point, we think, Dr. Hammond has fallen into an error. He states that along with an increased quantity of blood in the vessels of the brain there is sometimes found a large amount of sub-arachnoid effusion, and that the ventricles may also contain an excessive amount of fluid. This combination of an increased amount of blood with an increased amount of the cerebro-spinal fluid is, we believe, not found except in cases of long-standing or frequently-repeated congestion, in which there was reason to believe that a certain degree of atrophy of the brain existed. Nor can we see how such a condition can exist. We have even been struck in

the *post-mortem* examinations of cases in which there was evidence of anæmia, both from the symptoms before death and from the bloodless appearance of the brain on inspection, by the fact that there has frequently been no excessive amount of serum in the ventricles or in the sub-arachnoid spaces.

In connexion with this portion of the subject we may refer to a point on which we think the author has been led by physiological considerations to misinterpret an important symptom. By experiments on animals, with the aid of the cephalo-hæmometer, Dr. Hammond has convinced himself, and been able to demonstrate, that the amount of the blood circulating in the cerebral vessels during sleep is much less than during wakefulness. The instrument employed in these experiments is described as follows:—

“Although this instrument is intended for experiments on the lower animals, it enables us to arrive at very definite conclusions relative to the condition of the cerebral circulation. I first described it in a paper read before the New York Medical Journal Association in 1868, and shortly afterward published in the *New York Medical Gazette*. It was devised in somewhat different form, independently of each other, by Dr. S. Weir Mitchell and myself. The instrument consists of a brass tube which is received into a round hole made in the skull with a trephine. Both ends of this tube are open, but into the upper end is secured another brass tube, the lower opening of which is closed by a piece of very thin sheet India-rubber, and the upper opening by a brass cap, into which is fastened a glass tube. This inner arrangement contains coloured water. To this glass tube a scale is affixed.

“This second brass tube is screwed into the first till the thin India-rubber presses upon the dura mater, and the level of the coloured water stands at 0, which is in the middle of the scale. Now, when the quantity of blood in the brain is increased, the liquid rises in the tube, being pressed upward by the elevation of the thin rubber closing the lower opening; when the quantity of blood is lessened, the liquid falls by its own gravity.”

Dr. Hammond's results corroborate the views originally expressed by Durham in his paper on the physiology of sleep, in *Guy's Hospital Reports*, and confirmed by the ophthalmoscopic observations of Dr. Hughlings Jackson. Influenced by this consideration, Dr. Hammond states that the presence of drowsiness is a sign which may be relied on as pointing to cerebral anæmia, while wakefulness accompanies active cerebral congestion.

“In all cases of cerebral anæmia there is more or less drowsiness, from

the profound syncope of the rapid form to the rather agreeable languor present in slight cases. In instances of medium severity the patient readily falls asleep in the sitting posture, but recumbency induces wakefulness from the fact that the quantity of blood in the brain is thereby suddenly increased above the habitual standard, and a state of comparative hyperæmia is thus induced."

Now, the experience of many states in which an anæmic condition of the brain must exist is not favourable to this view, and the influence of many therapeutic agents is decidedly opposed to it. In the familiar instance of uterine hæmorrhage after delivery, if the loss of blood has not reached a degree sufficient to produce torpor of the faculties, and imminent danger to life, the effect is rather to engender wakefulness. A similar sleepless condition is often a concomitant of anæmia in young women, and the soporific effect of narcotics, alcoholic and other stimulants, and of warm food at bed-time, seem also to contradict this supposition. In fact, while we may freely admit the physiological fact, in favour of which there is certainly strong evidence, we have as yet no right to endeavour to explain the symptom of sleeplessness by a reference to it alone.

The following observations on some of the most important remedies used in brain disease will be found deserving of attention:—

"Bromide of potassium can almost always be used with advantage to diminish the amount of blood in the brain, and to allay any excitement of the nervous system that may be present in the sthenic form of insomnia. That the first-named of these effects follows its use, I have recently ascertained by experiments upon living animals, the details of which will be given hereafter. Suffice it now to say that I have administered it to dogs whose brains have been exposed to view by trephining the skull, and that I have invariably found it to lessen the quantity of blood circulating within the cranium, and to produce a shrinking of the brain from this cause. Moreover, we have only to observe its effects upon the human subject, to be convinced that this is one of the most important results of its employment. The flushed face, the throbbing of the carotids and temporals, the suffusion of the eyes, the feeling of fulness in the head, all disappear as if by magic under its use. It may be given in doses of from ten to thirty grains, the latter quantity being seldom required, but may be taken with perfect safety in severe cases.

"Since then, experiments with the cephalo-hæmometer and ophthalmo-

scope have abundantly confirmed these views, and more extensive experience in the treatment of cerebral congestion has placed the matter beyond the possibility of a doubt. Other observers have also confirmed the opinions here expressed.

"The prescription which I usually employ consists of bromide of potassium, \mathfrak{z} j.; water, \mathfrak{z} iv.; of this a teaspoonful is taken three times a day in a little water. Occasionally the bromide is increased to \mathfrak{z} iss., and sometimes a saturated solution—which contains grs. xxx. to \mathfrak{z} j.—is used. I continue the medicine till drowsiness, a slight feeling of weakness in the legs, and contraction of the blood-vessels of the retina—detected by the ophthalmoscope—are produced. The more prominent head-symptoms generally disappear in four or five days, and the results above-mentioned ensue in about ten days.

"Latterly I have used the bromide of sodium in corresponding doses instead of the bromide of potassium. It is more pleasant to the taste, and does not cause so much constitutional disturbance as sometimes follows the administration of the bromide of potassium in large doses.

"In conjunction with one or other of the bromides mentioned, I very generally employ the oxide of zinc, which experience has taught me is a powerful agent in relieving cerebral congestion, and giving tone to the nervous system. It should be given in doses of grs. ij., three times a day, either in the form of a pill or a powder, and to avoid any nausea should be taken after meals. At the end of about ten days it will generally be found that all symptoms of congestion—subjective and objective—have disappeared, leaving a little debility and mental depression. It then becomes expedient to give tonics and restoratives, and those which have a special action on the nervous system are to be preferred. Among them, strychnia, phosphorus, and cod-liver oil, stand first.

"Strychnia may be advantageously administered in conjunction with iron and quinine dissolved in dilute phosphoric acid, as in the following formula: strychniæ sul. gr. j.; ferri pyrophosphatis, quiniæ sul., $\bar{a}\bar{a}$, \mathfrak{z} j.; acid. phosp. dil., zingiberis syrupus, $\bar{a}\bar{a}$, \mathfrak{z} ij. M. ft. mist. Dose, a teaspoonful three times a day in a little water. I prefer this extemporaneous prescription to any of the syrups or elixirs with like ingredients. If for any reason the iron and quinine are not indicated, the strychnia can be given alone with the dilute phosphoric acid.

"Phosphorus almost always acts well in such cases as those under consideration. It may be given in the form of the phosphorated oil, as in the following formula: \mathcal{R} . Olei phosphorat., \mathfrak{z} ss.; mucil. acaciæ \mathfrak{z} j.; olei bergamii, gtts. xl. M. ft. emulsion. Dose, gtts. xv. three times a day. A very elegant preparation of phosphorus is the phosphide of zinc. . . . My experience with this medicine has been very extensive. I have never known it to produce the least unpleasant effect, and have rarely been

disappointed in obtaining the full results to be expected from phosphorus in corresponding doses. I am, therefore, not in accord with Dr. M. Clymer on this point.

"The chemical formula of the phosphide of zinc is $P. Z_{n3}$, and consequently a grain represents a little more than one-seventh of a grain of phosphorus. The proper dose, therefore, is about the tenth of a grain. I usually prescribe it in cerebral congestion, according to the following prescription: \mathcal{R} Zinci phosphidi, grs. iij.; rosar. conserv., q. s. M. ft. in pill, No. xxx. Dose, one three times a day. Instead of the conserve of roses, grs. x. of the extract of nux-vomica may be substituted if strychnia is not being administered in some other form."

Dr. Hammond's experience on the subject of the changes of the brain substance in tubercular meningitis does not correspond with that of observers in this country. He states that the tissue of the brain is not generally much involved in this disease; on the contrary, it has been observed that softening of the central portions of the brain is a very common occurrence in connexion with this disease, and frequently occurs to a very considerable extent.

The author devotes an elaborate chapter to spinal irritation, and puts forward some views which possess considerable ingenuity, and which are worthy of attentive consideration. He regards spinal irritation as one of the manifestations of anæmia of the cord. If the defective supply of blood be confined to the posterior columns, its effect is the production of the different morbid phenomena which has been generally referred to spinal irritation; if to the antero-lateral columns, paralytic phenomena are produced of the kind described as reflex paraplegia. The grounds on which the author relies for the support of his theory are these:—

"I have already stated it as my opinion that the essential condition of spinal irritation is anæmia of the posterior columns of the cord. Other writers have ascribed it to inflammation, congestion, hysteria, and numerous other factors. The reasons which have induced me to arrive at this conclusion are briefly as follows: Owing to the fact that spinal irritation is not *per se* a fatal disease, we rarely have the opportunity to verify any views we may hold in regard to its pathology. In the few cases in which *post-mortem* examinations were made, nothing abnormal was found, a circumstance, however, far more compatible with the idea I have expressed than with any other:

"1. It is a well-recognized fact that irritation is often a result of a deficient supply or a poor quality of blood. Thus headaches are frequently caused by cerebral anæmia, and are promptly relieved by increasing the

amount of blood in the cerebral blood-vessels. Irritability of the mind is also a constant accompaniment. A feebly-nourished stomach rejects food, and is the seat of pain. An anæmic heart beats with great rapidity, weak muscles are affected with tremor, and an exhausted generative system is brought into a state of unnatural erethism by the slightest kind of excitation. Analogy, therefore, supports the theory I have suggested.

“2. The diagnosis of diseases of the spinal cord has become so perfect that we are able to distinguish congestion, meningitis, myelitis, softening, tumours, etc., by their symptoms and by the means of research at our command. We see, therefore, that the morbid phenomena which result from such conditions are not such as we now class under the head of spinal irritation. This division of the subject will be more fully considered under the head of diagnosis.

“3. I have repeatedly ascertained, by actual experience, that those agents which are known to diminish the amount of blood in the spinal vessels invariably increase the severity of the symptoms due to spinal irritation, while they are as effectually lessened in intensity by remedies which tend to produce spinal hyperæmia.

“4. The general condition of patients the subjects of spinal irritation is always below par, and the exciting causes are all such as tend to the production of asthenia.

“5. The character of the symptoms points decidedly to the greater, and at times sole implication of the posterior columns. There are cases of the disorder in which there is no derangement of motility in any part of the body, and in all cases aberrations of sensibility are the prominent features. Moreover, the viscera are generally affected in their functions, a circumstance of itself strongly indicative of the situation of the lesion in the posterior columns.”

It will be easily seen that there is by no means sufficient evidence adduced to support the position adopted by the author, and one can only regard his remarks as a contribution to the elucidation of a very obscure subject.

On the subject of sclerosis we have a good summary of the recent views of the French pathologists.

Dr. Hammond believes that, although chorea coincides in point of time not unfrequently with rheumatism, there is no connexion pathologically between the two diseases further than that the depressed state of the system induced by the one may pre-dispose to the other. His own figures, however, point to a different conclusion, for out of 82 cases of chorea he found sixteen to be connected with rheumatism—a proportion which certainly cannot

be the result of accident. In the treatment of chorea the author relies mainly upon strychnia, and he has found the application of the ether spray to the skin to be decidedly beneficial.

Of the important subject of neuralgia we have but a meagre account. The author, with justice and in accordance with the views of most recent authorities on this point, recommends strongly the constant current as the best therapeutic agent at our disposal. He gives, however, no account of the varieties of neuralgia in which it is most serviceable, and he seems to have employed it somewhat indiscriminately, and always in connexion with constitutional treatment.

We cannot close our remarks on this important work without observing that we entertain a high opinion of the extent of knowledge as well as of the general ability and judgment displayed by the author in its production; and we hope that in future editions it will be rendered still more worthy of the confidence of the professional reader.

RECENT WORKS ON URINARY DISEASES.

1. *The Skim-milk Treatment of Diabetes and Bright's Disease, with Clinical Observations on the Symptoms and Pathology of these Affections.* By ARTHUR SCOTT DONKIN, M.D., Edinburgh; M.D., Durham; Lecturer in Medical Jurisprudence and Toxicology in the University of Durham, &c. London: Longmans, 1871.
2. *Remarks on Diabetes, especially in reference to Treatment.* By WILLIAM RICHARDSON, M.A., M.D., Member of the Royal College of Physicians, London. London: Lewis, 1871.
3. *A Practical Treatise on Bright's Diseases of the Kidney.* By T. GRAINGER STEWART, M.D., F.R.S.E. Second Edition. Edinburgh: Bell and Bradfute, 1871.

DR. DONKIN has embodied in this volume the substance of his contributions to the treatment of diabetes, which originally appeared in the *Lancet*, and which have already been noticed in this journal. He has, in addition, entered somewhat fully into the question of the pathology of diabetes, and given a tolerably full account of this disease. The mode of treatment which he has recommended con-

sists in the administration of six or seven pints of skim-milk daily. This quantity is to be divided into four meals, with an interval of four hours between each. It is essential to this plan, that no other article of food whatever should be taken by the patient.

Dr. Donkin's views regarding the relation of the oleaginous or fatty principles of food to diabetes are novel, and are urged with some force. He holds, contrary to the received doctrine on the subject, that fats and oils are capable of being transformed into diabetic sugar, and, as a consequence, would exclude articles of this kind from the dietary of a diabetic patient. It is from this cause that he regards it as necessary that the greater part of the cream should be removed from the milk, which forms the staple of his treatment. In one case of diabetes treated on Dr. Donkin's plan, he found that allowing the milk to stand twenty-four instead of twelve hours before being skimmed, so as to ensure a more complete separation of the cream, was followed by a marked decrease in the specific gravity of the urine, and by a diminution in the sugar to one half of its previous quantity. In another case, the administration of new milk, rich in cream, caused a return of sugar in the urine about a fortnight after its disappearance under the skim-milk treatment. Dr. Donkin's opinions on this subject are opposed to those of Dr. Pavy, and are directly contrary to the observation of Dr. Richardson, the title of whose work will be found at the head of this notice. This author states that fat ought to be given in large quantities: cod-liver oil when it agrees, but if it be unsuitable, as much cream or fresh butter as can be digested. Cod-liver oil has been found to be very beneficial in a case observed by a no less accomplished chemist than Dr. Bence Jones.

As regards the nature of diabetes Dr. Donkin suggests that it may depend on perverted functional activity of the liver cells, whereby they morbidly secrete diabetic sugar instead of glycogen, their normal secretion.

An advantage of some importance claimed for the skim-milk treatment is that it relieves thirst, and is pleasant to the patient, especially when first administered. It is, besides, a cheap mode of treatment. The effect of the treatment, according to the author, is remarkable for its rapidly beneficial results obtained, twenty-four hours being generally sufficient for the production of a marked improvement, and seldom more than from two to six days being required to procure complete relief from suffering. In cases of not long standing, the sugar will be completely removed from the

urine in a period varying from twelve days to five or six weeks. In cases of long standing, a temporary arrest of the disease will be brought about.

The author has adopted the same mode of treatment with great success in Bright's disease. The cases most amenable to the proposed treatment, he has found to be those in which the renal disease had begun in the inflammatory form, and afterwards passed into the chronic condition. The rules for the administration of the milk are the same as in diabetes.

The second work on our list contains the personal experience of a sufferer from diabetes. Dr. Richardson was attacked, as he informs us, ten years ago with diabetes. After a prolonged trial of the most approved remedies he was fortunate enough to hit upon a plan of treatment by which he has been cured, and by which also other diabetic patients have been much benefited. The essential features of this plan are—the employment of regular and steady exercise; ablution of the skin daily with soap and water; the use of a bath, containing a table-spoonful of carbonate of soda, twice in the week; exposure of the surface of the body as far as is practicable to sunlight; and the continuous use of iron, which he uses in the form of tincture of the perchloride in four or five-drop doses, with one or two drops of tincture of *nux vomica*, and eight or ten grains of chlorate of potash three times daily. He is an advocate of a restricted diet, but when the plan of treatment which he suggests is carried out fully he finds that a considerable amount of relaxation as regards food is not injurious.

He regards the sudden adoption of a very restricted diet as likely to prove highly prejudicial. Dr. Richardson's present dietary is sufficiently liberal, and besides meat, includes brown bread, with plenty of fresh butter, maccaroni, and rice, potatoes sparingly, and occasionally a little dry fruit. Even a few glasses of champagne occasionally he does not find at all injurious.

The second edition of Dr. Grainger Stewart's excellent work on Bright's diseases has received considerable additions and improvements. The clinical history of the different maladies grouped under this heading is more fully discussed, and a number of illustrative cases, many of which possess much interest, have been added. A new chapter on the differential diagnosis of the three forms of Bright's disease will be found of much practical

usefulness. Two additional illustrations are contained in this edition.

The sterling merit of Dr. Stewart's work has been already recognized by the profession. It embodies the conclusions of an accomplished and original observer, and may be consulted with confidence and advantage.

Pharmacopœa Norvegica. Editio Altera. Regiâ auctoritate Edita. Christianiæ 1870. Impensis Alb. Cammermeyer. Typis H. J. Jensen. Octavo, pp. 320.

THE second edition of the Norwegian Pharmacopœia has been compiled in accordance with a Royal Decree of the 2nd of February, 1867. In it are embodied the suggestions of a select Committee of Danish, Swedish, and Norwegian Physicians, which assembled at Stockholm, in 1865, for the purpose of emending the pharmacopœias of those countries. One of the most important alterations in the present work is the substitution of the metric system of weights and measures for the old medical pound and its subdivisions. In this particular the Norwegians have followed the example of their Swedish brethren, who adopted the metric system in the seventh edition of their Pharmacopœia in 1869. The number of remedies has been reduced considerably, some 250 preparations being omitted against only about 60 innovations. The work is in Latin, but the Norwegian Synonyms are given, as also those of the various foreign Pharmacopœias. A series of valuable tables and appendices conclude the work, which makes a handsome volume, with clear type and very accurately printed.

Observations upon the Climate of Uckfield, Prognostics of Atmospheric Changes, and some Vital Statistics. By C. LEESON PRINCE, M.R.C.S., Engl. ; F.B.M.S., &c. London: J. & A. Churchill. Lewes: Geo. P. Bacon, 1871. 4to, pp. 242.

OUR author possesses in a high degree a quality essential in one who aspires to eminence as either a meteorologist or a medical man—namely, the power of accurate and persevering observation. In the present volume Mr. Prince has included the results of a series of meteorological records, extending over 27 years, and

apart from any intrinsic value of the book, the very fact that it is based upon so lengthened a period of almost constant observation, should attract attention. Uckfield Observatory is situated 144 feet above the level of the sea, in latitude $50^{\circ} 58' 25''$ N., and in longitude 24 seconds East of Greenwich. The climate described is, therefore, that of the South-eastern part of England.

Having devoted a chapter to the subject of the barometer, the author proceeds to a consideration of the general meteorological character of the several months of the year, introducing each month with a very appropriate poetical quotation. The question of temperature is fully illustrated by tables and remarks. Chapter III. is devoted to the hygrometric conditions of weather, including moisture, rain-fall, direction and prevalence of winds—the last-named subject being very properly dealt with in connexion with hygrometry.

Chapter IV. is, perhaps, the longest we have ever met with, extending, as it does, over 134 pages of the book. But it is very readable, and contains a most interesting account of the leading atmospherical phenomena, noticed in each month from January, 1843, to December, 1870, both inclusive. The writer of this Review, as a meteorologist himself, may be allowed to congratulate Mr. Prince on the manner in which he has worked out this portion of his book.

In a fifth chapter we meet with a complete history of weather prognostics, beginning with the *Diosemeia* of Aratus, including the prognostics given by Virgil in the *Georgics*, and ending with those derived from observation of scientific instruments, of natural phenomena, and of the habits of birds, animals, and plants.

From what we have gleaned of Mr. Prince's character as an observer, the scanty size of Chapter VI., in which the all-important topic of vital statistics is touched upon, tends to produce a feeling of disappointment in our minds that more abundant data and fuller opportunities for studying this subject were not within our author's reach.

A Dictionary of Chemistry and the Allied Sciences. By HENRY WATTS, B.A., F.R.S., &c. Supplement. London: Longmans, Green, and Co. 1872.

WATTS' Dictionary of Chemistry is the largest, most systematic, and, we would venture to add, most accurate work on the science

of chemistry which has ever issued from the British press. Exclusive of a supplement just published, it consists of five very large volumes, containing several thousands of pages of close type. It is indeed a work of which not only its able editor, and its enterprising publishers, but the whole scientific public of these countries, may feel justly proud. The Dictionary is not confined to chemistry proper, for a large proportion of its subject-matter relates to those branches of the cognate sciences which merge into, or are intimately connected with chemistry. With Watts' Dictionary and a good work on analytical chemistry in our possession, we could well dispense with any other works on chemical science.

The Supplement which now completes Mr. Watts' arduous undertaking is a large volume of 1,136 pages. It brings down the record of chemical discovery to the end of the year 1869, but it also notices many new facts discovered in 1870, and even in 1871—as, for example, Bunsen's experiments to determine the atomic constitution of the metal Indium, the determination of the specific gravities and boiling points of some normal amyl compounds by Rossi, &c. The Supplement also contains numerous corrections of statements made in the earlier volumes. We need hardly add that every owner of the Dictionary of Chemistry should supply himself of a copy of its Supplement.

WORKS ON HYGIENE, ZYMOTICS, &c.

1. *Remarks on the Prevalence and Distribution of Fever in Dublin.* By THOMAS W. GRIMSHAW, M D., &c. Dublin: Fannin. 1872. 96 pp.
2. *First Biennial Report of the State Board of Health of California, for the Years 1870 and 1871.* Sacramento. 1871. 113 pp.
3. *First Annual Report of the Board of Health, of the Health Department of the City of New York—April, 1870, to April, 1871.* New York. 1871. 628 pp.
4. *Zymotic Diseases ; their Correlation and Causation.* By A. WOLFF, F.R.C.S. London: Churchill. 1872. 1,177 pp.

THESE works will be specially referred to in the Report on Public Health, which will appear in our next number.

PART III.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

Wednesday, March 20th, 1872.

DR. HUDSON, President of the College, in the Chair.

The discussion on Dr. Foot's paper, on the antiseptic treatment of small-pox, was resumed by

Dr. GRIMSHAW, who said he had listened to Dr. Foot's paper with great interest, more especially as it possessed peculiarities which were not found in all papers on therapeutics. It stuck to one point, and applied a special form of treatment to a special form of disease from a special point of view. He (Dr. Grimshaw) was not a great friend of the antiseptic treatment of contagious febrile diseases. He had tried it very largely in various forms of the febrile disease, especially scarlatina, typhus fever, and enteric fever. Those, however, differed very considerably from small-pox, and if there was any disease in which they might expect benefit from antiseptic treatment, it was small-pox—he spoke, of course, of antiseptic medicine, which must be distinct from antiseptic surgery. In the one case they had a healthy condition, from which their object was to intercept the disease—whereas, in the other the disease had got into the system, and they endeavoured by antiseptic treatment to destroy it. In small-pox they had two diseases to deal with—the poison that had entered the system, and the disease that had been set up by the action of that poison. They had primary fever and secondary fever, and it was the latter which was most likely to be influenced by the antiseptic measures. His experience was altogether against antiseptic measures. He had treated a large number of cases with antiseptics, and in no case did he find that the amount of stimulants required was less than that required

in cases not treated antiseptically, and this he looked upon as being in a great measure the test of the success of the treatment. He had not probably tried the antiseptic treatment as extensively as Dr. Foot; but he had tried it in some thirty-five or forty cases, and he had that day gone roughly over the cases that were thus treated in the Cork-street Hospital. There were twenty-three cases which he treated with sulpho-carbolate of iron, which he looked upon as having a *prima facie* character of being the best antiseptic medicine. Of those cases, there were ten which he might almost throw out, because, although at first they set in furiously, he thought, from what he knew of the disease, whether treated antiseptically or not, they would have turned out well. That left thirteen cases. Of those three died, two of them being purpuric, and one ordinary confluent small-pox. When he compared this with the bulk of the cases he had treated by other methods he did not find any difference. He did not rely altogether on antiseptic treatment in those cases. There were some of them in which he had used stimulants in very considerable quantities, in some cases administering 20 ounces of wine and 8 ounces of whiskey in the 24 hours. He was sorry Dr. Foot had not said more of the extent to which he had used stimulants, for he took it for granted, stimulants had been used to some extent. His experience then was that the antiseptic treatment combined with stimulants was not more favourable in its results than the other treatment which he generally adopted—perchloride of iron and stimulants combined. He could not see much difference between them. He thought stimulants were the great points. The local lesions most dangerous in the present epidemic were the throat lesions—these he had treated by nitrate of silver, by leeching the throat, by gargles and washes—which, perhaps, more than anything else, gave comfort to the patient. The remedy he found most useful in throat cases was the application of leeches externally over the angle of the jaw, or over the larynx, when the larynx was affected. Dr. Foot had mentioned delirium. Now the treatment of delirium was not an easy matter. In most of the cases he had met with, it was of a troublesome, though not dangerous character; but though the rule, this was not always the case. They had had in Cork-street, and also in Steevens' Hospital some most furious cases, some which threatened destruction to all around them. One patient assailed every one he could get near. Another patient absolutely barred the door on the nurse, smashed the bars of the window, and jumped out a distance of some 18 feet. These were most difficult cases to deal with, and although he agreed with Dr. Foot in opposing all kinds of physical restraint in cases where patients were not furious, yet he did not see any other way of treating such patients as he (Dr. Grimshaw) had described, except by physical restraint. They could not allow a patient to injure others, perhaps to injure himself, or to damage the furniture of the hospital if nothing else; and he thought it the most merciful plan towards the patient to secure him at once

from doing mischief. If they adopted the plan of having a number of attendants who were constantly going at the unfortunate individual and hunting him back to bed, it generally ended in a fight, in which the patient might be injured; whereas, if he were caught at once by two persons accustomed to this duty, and tied in his bed, he could not injure himself or any other person, and, if moral influence had any power over him, he would be generally found to submit to the treatment. He had found restraint of this sort for half-an-hour had been quite successful in quieting the patient ever afterwards. When he got a furious patient secured in his bed, he applied a couple of leeches to the temples, and gave him a dose of chloral. The patient then went asleep, and while asleep the tyings were removed, and it was unnecessary to restore them. Dr. Foot had referred to purpuric cases, which, from the time of Sydenham downwards, were considered hopeless cases. Since Dr. Foot's paper was read he had seen instances of recovery in purpuric cases. They were, no doubt, rare; they were never of the most intense form, but he had seen some with black spots and hæmorrhage, in which there was recovery. The plan he had adopted in those cases was the administration of a considerable amount of stimulant combined with turpentine and ergot of rye. In one case, that of a woman, ordinary stimulant had failed, turpentine had failed, but when ergot of rye was adopted, it seemed to take effect at once, the hæmorrhage was stopped, and the patient rallied, and subsequently died from an affection in her throat, and not from debility. He had found other cases rally when ergot was administered. In one case of violent uterine hæmorrhage, where the woman had been delivered while suffering from small-pox, the patient recovered. At present he had a patient who was admitted with black spots. He had expectorated blood, and had passed some blood from the bowels and from the nose. That patient was now convalescent. This was the only case in which there were purpuric spots and hæmorrhage combined that he had seen recover. In another case which he treated by zylolæ, the black spots disappeared, but the patient died. It was quite possible, therefore, that these cases which were generally considered hopeless might recover. Statistics of treatment, however, in diseases of this nature were very unsatisfactory, and for his part, he did not think them worth a button. He wished to impress on the members of the Society the value of leeches applied to the temple, the head, the angle of the jaw, and above the larynx. The only special point on which he should wish to hear some observations from Dr. Foot was that of alcoholic stimulants. The furious delirious cases to which he had referred were persons who had been previously addicted to habits of intoxication. A great many of the purpuric cases were of the same class of people. The most terrible case of disease he ever saw in his life was a case of a man whom he had twice treated for delirium tremens. This man became the most frightful subject of small-pox he ever saw. There was not a particle of cuticle that

did not separate from his body, and the extraordinary thing was that he should have gone on for 48 hours in a state of universal gangrene, and that he did not die within an hour or two after this terrible condition had supervened. Another point he wished to call attention to was that Dr. Foot had described his cases as those that were confluent, or as bad as if they were confluent. Now, there was a class of confluent cases which, if he might be allowed the expression, he would say were not as bad as if they were confluent—that was cases of vaccination where the rash came out confluent, but did not suppurate. The secretion assumed a sort of warty appearance, like elephantiasis. These cases were not dangerous, and they were liable to give rise to a mistake as to the result of treatment. Those were cases where the antiseptic treatment ought to prevent suppuration. He might mention that in some of the cases in which he tried the antiseptic treatment the patients had boils, which he thought the antiseptic treatment should have controlled.

Dr. DARBY said his experience of this disease coincided altogether with that of Dr. Grimshaw, and he agreed with him from first to last in everything he had said. He entirely approved of his suggestion about leeching. He did not find the delirious cases were as fatal as those in which there was no delirium at all; and he did not, therefore, think delirium was a bad sign. He agreed with Dr. Grimshaw as to the necessity of timely restraint. It was much less of a shock to the patient to be put into a straight waistcoat or tied down by sheets than to have a struggle with four or five nurses, and when the former method was adopted, the patient became quiet in a much shorter time. With regard to purpura, he had not seen many cases, but he had never seen a case recover in which there were both hæmorrhage and black spots. He had, however, seen some recoveries in cases in which there were black spots only. He did not believe in the antiseptic treatment at all. There was only one disease in which general septicemia occurred. He had of course seen gangrene of large portions of the body occurring from various causes, but the disease to which he referred was purpuric fever, which he believed was septic. He had seen a patient having a gangrenous smell, and be putrid before death; but with that exception, he never saw in the sick-bed while the patients were alive, or in the dead room after they were dead, any evidences of putrefaction beyond that which occurred in the ordinary course of nature. He did not think patients dying of typhus fever or small-pox putrefied in the dead-room sooner than if they died of other diseases. He had never, he repeated, seen putrefaction in the living subject with the one exception of purpuric fever, and in that case he had seen patients with the unmistakable physical signs and smell of gangrene before they were dead.

Dr. HAYDEN observed that the question before the chair was one of so much importance, that any one who had a modicum to add, however small, to the sum of knowledge, should not hesitate to make his contribution. The questions raised in Dr. Foot's admirable paper were of such importance, that they must of necessity fix the attention of the profession. He should commence, in discussing very briefly the substance of the paper, by expressing his entire assent to the observations made by his friend Dr. Grimshaw, in reference to the antiseptic plan of treatment. He had tried it pretty largely and was not satisfied with the result. He had tried it in the form of sulphurous acid, sulpho-carbolate of sodium, sulpho-carbolate of iron, and he could not say he was satisfied with the treatment; in the majority of instances these agents sickened the patients, and rather than forego the benefit of nutriment and stimulants, he was obliged to discontinue their use. He fell back ultimately on the old plan of treatment by perchloride of iron, in doses of from 15 to 30 drops largely diluted every second hour, and under this plan of treatment some of the patients had recovered. Dr. Foot said the principal cause of death, especially in confluent cases was laryngitis. He agreed with him, but he did not think all the fatal cases set down as laryngitis were fairly attributable to that special cause. He had seen several cases die of simple asthenia. He had not had an opportunity of examining the condition of the muscular substance of the heart in those cases, but he had a strong suspicion that it would have been found in a condition that would afford a satisfactory explanation of the failure of the circulation, and death in that way. He had known several cases of the kind. The patients had had a fairly good pulse on the last day he saw them, and had swallowed liquid food moderately well, the breathing was not embarrassed, and on the following day he found them dead. That had occurred most frequently at the period of secondary fever—that is, when the pustules were in process of disappearing, which would be scarcely consistent with the view that œdema of the glottis could have set up so rapidly as to suffocate the patient in a period of 24 hours. Delirium he had frequently witnessed, and he was quite of the opinion that the best remedy for it was the application of leeches to the temple in many of the cases. As a soporific he had found bromide of potassium and opium combined the most effectual—20 grains of the former and 8 to 10 minims of the liquor morphiæ hydrochloritis of the Pharmacopœia, of the latter. He could not agree with Dr. Grimshaw that chloral was an agent that would succeed in these cases. He had found it aggravate the delirium. Opium also failed, and the only treatment he had found of use was leeches to the temple, and a combination of bromide of potassium and morphia. He agreed with Dr. Foot when he stated that the hæmorrhagic form of the disease appeared to disregard re-vaccination. The very worst cases of this kind he had seen were

cases that had been previously vaccinated. As to the number and fatality of hæmorrhagic cases, Dr. Grimshaw had raised a point of much moment. There appeared to be an impression that a hæmorrhagic case was necessarily fatal, but his experience did not sustain that view. He had had 17 hæmorrhagic cases under his care, in all of which hæmoptysis, hæmatemesis, or menorrhagia were combined with discolouration of the surface—sometimes large bullæ, and the surface coloured purple especially on the neck, legs, and hips. Of these cases four had recovered and thirteen had died. The treatment he had invariably adopted in these terrible cases was perchloride of iron. At present he had two hæmorrhagic cases which he was treating with chlorate of potash, in order to compare the result with those of the treatment by perchloride of iron.

Dr. DAVYS observed that one remark had fallen from Dr. Grimshaw which, as a reader of the *Medical Press and Journal*, he should not like to go abroad without protest—that statistics in disease were not worth a button. [Dr. GRIMSHAW—I said statistics of treatment.] Dr. Grimshaw, however, contradicted himself. He told them that he had not had much experience of treatment by zylol, and therefore could not tell them the results of it. If he had had any results of such treatment he would have been happy to lay them before the Society, consequently attaching a certain degree of value to statistics in disease. Unhappily, in his portion of the country he had seen a good deal of small-pox, and the idea he had formed of the affection of the throat was that it was a form of diphtheria, resulting from a poisoned condition of the blood, and he had found very great benefit in such cases from local applications of muriate of iron. In every case in which leeches were applied, the condition of the patient was lowered, the disease was greatly aggravated, and the patient died.

Dr. HENRY KENNEDY said he could not but express his surprise at the views which the gentlemen who preceded him had advanced in reference to the antiseptic treatment of disease, for they had said as much as that they had no faith in it in the treatment of small-pox—in fact, they spoke as if such a plan did not exist at all. If this were so it would indeed be hard to understand the treatment of most diseases at the present day—as, for instance, our common fevers. For himself, he had the fullest faith in this plan of treatment. He looked upon wine as antiseptic, and bark, and the mineral acids; and the medicine which Dr. Hayden had just spoken of—the tincture of perchloride of iron—what could it be considered but as an antiseptic. Dr. Kennedy then went on to say—“I believe that the treatment of the present epidemic ought to be essentially of the same character; and this leads me to speak of one of

the new medicines which have been used for the disease—I mean the sulpho-carbolate of sodium. This is a medicine I would not use. I believe that the soda contained in it cannot but be injurious. Physiology has taught us that alkalies impoverish the blood, and, on the contrary, that acids improve it. Besides, I have found by experience that the sulphites and hypo-sulphites act badly in our common fevers. Hence I have been led latterly to use nothing but the acids, and amongst these I would specially name the sulphurous, which seems to me to be very useful, and which Dr. Foot has also so extensively used. In the hæmorrhagic cases I do not, however, trust to it alone, but join with it one handed down to us from the time of Sydenham—the dilute sulphuric acid in full doses; to these again I add laudanum in small doses—that is, from three to five drops in each dose; nor have I the slightest doubt that it produces a soothing and beneficial effect. Out of two cases, in private, of the hæmorrhagic class, one was saved. It was a case I saw with Dr. Wyse, and the bleedings from the chest and uterus were very considerable, whilst the body presented a very large number of purpuric spots. In the second case, which Dr. Stokes saw, the lady died on the fifth day from the first appearance of the rash, the bleedings being most general and profuse. On the use of aperients I would venture to advise great caution, even at the commencement of the attack; but still more so if the patient's life be prolonged to the ninth or tenth day, for then, as is well known, secondary fever is very apt to occur, and an aperient may determine the matter one way or other; for if the blood-vessels be kept full, as was long since shown by Magendie, absorption may almost be rendered nil; but if they be partially emptied the absorbents will at once become active, and mischief must result. A similar remark, too, applies to cases of puerperal fever, where hæmorrhage is known to be a strong predisposing cause of the disease. In the cases of small-pox fatal from the secondary fever, I can bear out Dr. Hayden's remark, that they die from asthenia, and not from any affection of the larynx."

Dr. JOHN HUGHES thought Dr. Foot deserved well of that Society for directing attention to the subject of small-pox, and he had done so in a communication no less conspicuous for its ability than its candour. What he understood Dr. Grimshaw to mean by his reference to statistics was that they were premature at a time when it might be said the epidemic was in full swing, and when it was impossible to collate them with sufficient accuracy and care. Dr. Foot had brought forward 30 cases. Of these six were semi-confluent, and they all knew these cases would naturally terminate favourably without any treatment at all. That left 24 confluent cases. Now it would clear the ground if Dr. Foot would say how many were simple cases, that is, were confluent on the face and extremities only, and how many were confluent all over the body; for

from his (Dr. Hughes') experience the confluence of the eruption on the trunk as well as on the face and extremities had much more influence in determining the issue of the cases than any treatment that could be adopted. He regretted to say he had seen no case terminate favourably in which the disease was confluent all over the body as well as on the face and extremities.

Dr. LYONS said they had heard a great deal about antiseptic treatment, and as he thought the word was misleading them, he had got up from the library two standard dictionaries, in which they would find, as they all knew, that septic was derived from the Greek word sepo, to putrefy, and that antiseptic meant something that would prevent putrefaction. Now, so far as he was aware, without a single exception, the tendency to a strictly putrefactive process was one extremely rare in the human body, and was certainly extremely rare in small-pox; and, speaking physiologically and pathologically, the process which was taking place in the system in the development of small-pox was not putrefactive. The putrefactive process is one that runs to the death of the minute tissues, and suspends the formation in any very active degree of suppurative cells. Now one of the features of this disease of small-pox was to develop all over the system the formation of suppurative cells. He would be sorry it should appear to others outside that in that Society they did not follow a very close, rigid, pathological definition in their observations; and he hoped the gentlemen who had used the words antiseptic treatment so very largely would pardon him for this attempt to draw their attention to the necessity of a more strict and logical definition of what they meant. He thought Dr. Foot had in view a particular phase of the disease in which a septic process may exist, and where medicines known to have an antiseptic influence may have a beneficial effect; but if the words were to be used, they should be used with strict logical accuracy, and they should not go before the British public and leave it in the power of any one to say, as was sometimes said of Irishmen, that in discussing an important question they used words which they did not exactly mean.

Dr. GORDON said he had used the sulpho-carbolate of sodium in a great number of cases of small-pox, but had confined his use of it to those cases in which in the early stage of variola there was evidence of remarkable features of blood disease. He did not allude to purpura or hæmorrhage, but such complications as did not belong to small-pox itself, such as congestion of the brain, or lungs rapidly supervening in a very early stage of the disease. It is always difficult to decide how much the success of the treatment is due to the administration of any particular medicine in an early stage of the disease, and the only way in which it can be determined is by carefully investigating how far particular symptoms

are directly influenced by any particular medicine. He found that after the administration of sulpho-carbolate of sodium there were two prominent results, so remarkable in many cases, that he looked upon it as cause and effect, their being produced by that medicine. These were diminution of pulse and reduction of the temperature of the body. In many of these cases the symptoms were most alarming, and the progress of improvement was so rapid that in 48 hours after the administration of the medicine he was able to treat the disease as small-pox *per se*, uncomplicated. As to laryngeal affections, which, in his opinion, had been the complication that had proved most fatal in the present epidemic, he had found leeching most successful, sometimes followed by counter-irritation, but always by poulticing kept up, as Dr. Stokes said, like a local vapour bath—poulticing in this way after the leeching proved most effectual. His experience had been different from that of his friend Dr. Darby as to the result of treatment in cases of small-pox complicated with hæmorrhage and purpura. He had found most remarkable benefit in such cases from perchloride of iron, and he could point to more than one case in the Hardwicke Hospital where recovery had taken place under these circumstances. A man had been admitted recently who was advanced in life, with purpuric spots very much diffused and a considerable discharge of blood per anum, and yet the patient recovered. The time of the occurrence of the hæmorrhages was most important as to prognosis. If they occurred before the development of the variola, they were almost always fatal, but if they occurred later recovery was not unusual. The delirium in cases of small-pox he looked upon as not very unfavourable except in those cases where it caused loss of sleep for a long time. In such cases he had found leeching of great use, but in the more severe cases he had found treatment with tartar emetic, sometimes combined with opium, very beneficial.

Dr. HAWTREY BENSON said that no allusion had been made to a form of treatment so prominently brought before the Society on a late occasion by Dr. Stokes. He referred to the treatment by the bath. He was so struck by the result in Dr. Stokes's cases that he determined to adopt the treatment in the next suitable case he met. In a very few days such a case presented itself. The patient was a gentleman residing in one of the suburbs of Dublin. He suffered from an extremely bad form of confluent small-pox. It was remarkably confluent, not only on the face, but also on every part of the body. The pustules were not well filled, but were flat, and the face presented the appearance as if a wax candle had been dropped over every part of it. During the secondary form the delirium became extremely troublesome, and the patient quite uncontrollable. For the previous twenty-four hours he had not been in bed for five minutes, and he had had no sleep for over thirty-six hours.

Hypnotic remedies had no effect, and it was not possible to apply leeches or other applications to the head. With some difficulty he was placed in a slipper-bath, of the temperature of 98°, and he immediately exclaimed, "its glorious—its delicious—its delightful." He became at once calm, collected, and obedient, and within fifteen minutes he ceased to have any delirium. After half an hour he slept in the bath for two hours, occasionally waking for a minute or two while fresh water was being added. He (Dr. Hawtrey Benson) kept the patient in the bath for five hours and a half, removing him after that on account of headache which supervened. He was then put to bed perfectly free from delirium, and with the help of 15 grains of chloral (of which four times that dose had no effect previously), he slept uninterruptedly for eight hours. The case progressed from that out without the slightest check. Dr. Hawtrey Benson thought that this treatment did not receive its due measure of attention at the hands of the profession.

Dr. GRIMSHAW asked what was the temperature of the man's body when he went into the bath.

Dr. H. BENSON replied that the temperature could not have been lower than 104, and the bath was at least 6 degrees lower.

Dr. LAW said that up to the 10th of the present month he had an opportunity of treating 76 cases of small-pox; and of these there were 8 fatal cases, and 5 of the latter were cases of delirium tremens; the eruption never came out properly, and the lower extremities were covered with purpuric spots. Of the others, some were severe hæmorrhagic cases. One case was a very remarkable one. He happened to meet Dr. M'Clintock on a Tuesday, and asked him to come to Sir Patrick Dun's Hospital. When Dr. M'Clintock saw the patient, he said "I know that face." "Oh, yes," said the man, "I saw you, Sir, in Fawcett's shop, in Henry-street, on Friday." The patient was walking about the shop on Friday, and in the evening he was brought to hospital. He never saw such profuse hæmorrhage from the lungs and bowels as this man had, and his body was covered with purpuric spots, and he died on Tuesday. If there were any remedy of use in such cases it was the perchloride of iron. Among the small-pox patients he had referred to there were only two children, three years old; the next in point of age was 8 years, and the next 14 and 16 years old, proving that vaccination did preserve its protective force up to a certain period; and that was to him the strongest proof of the value of vaccination. The fatality chiefly occurred among the unvaccinated patients. Vaccinated cases, no matter how severe, went on favourably. Less importance was to be attached to small-pox occurring in vaccinated cases, except where it occurred with delirium tremens, then a combination of tartar emetic and opium benefited the patient. The combination of bromide of potassium with opium had also a good effect. A woman was

under Dr. Smith's care with the suppuration well out on her, and on the second day she was confined of a healthy living child, and singular to say this was the only living child this woman had had out of four. He thanked Dr. Lyons for his definition of the antiseptic treatment. It was begging the question to assume that any such thing as a septic condition existed. He believed the blood never underwent decomposition as long as it remained in its vessels. The only condition in which it became decomposed was where it was extravasated into the lungs, or came into contact with the air in epistaxis, where the plugging of the nostrils was resorted to. That was the only case in which septicemia existed.

Dr. AQUILLA SMITH said that Dr. Hughes was the only speaker who had referred to the special object of Dr. Foot's paper, and he (Dr. Smith) felt pleasure in saying that the thanks of the profession were due to Dr. Foot for the zeal and diligence with which he had investigated the matter, although the title of his paper was a misnomer. When he (Dr. Smith) came to the meeting that evening he thought the discussion would be confined to the question of the antiseptic treatment. The view which Dr. Kennedy took of the antiseptic treatment reduced it to nothing. Good food and fresh air were antiseptics in his view. He repeated that he thought the term antiseptic was a misnomer. It was based on the hypothesis that the patient laboured under a septic poison, and they were not justified in such an assumption. Dr. Grimshaw said the object of the antiseptic treatment was to prevent suppuration. If that were so he (Dr. Smith) considered it the most fatal line of treatment that could be adopted. He had under his care at present a series of cases of small-pox, and he considered that one of the clearest indications of the healthy progress of the case was the suppurative stage following its due course. He had at that moment 25 cases under his care in hospital, and in that small number there was a wonderful range of difference of degree. A patient came in that day, whom a junior practitioner might be very well excused in mistaking for a person with chicken-pox merely, yet a very few days would show that it was small-pox. A girl came in apparently affected with chicken-pox; it was on her face only, but every one went through the suppurative stage. He had remarked that small-pox in vaccinated cases ran its course with great rapidity. With regard to the woman who had the child born in the hospital, the pustules matured only in the face. On the other parts of the body they dried up. He did not think any point was gained in therapeutic treatment by Dr. Foot's paper. In the 30 cases which he had the mortality was 11 or 36 per cent. Now he (Dr. Smith) found a table of mortality in Copland's Dictionary, prepared by Dr. Gregory, who had charge of the small-pox hospital in London, giving the mortality for ten years, ending in 1830; and it appeared that the mortality among the patients who were unvaccinated, or had bad

marks was 36 per cent. He did not believe in the antiseptic treatment ; but he thought they might elicit something useful if they confined the discussion to confluent small-pox ; that was the cases in which the disease was confluent over covered the whole body.

The PRESIDENT—Dr. Foot having said that he adopted the treatment of small-pox by sulpho-carbolate of sodium from seeing its effects in cases of diphtheria and scarlatina in my wards, I would wish to offer a very few remarks. I may mention that my attention was drawn to it by the perusal of a paper by Dr. Sansom, in the 52nd volume of the *Medico-Chirurgical Transactions*. I apprehend that Dr. Sansom does not put it forward as an antiseptic, in the sense in which the word is understood by some gentlemen who have spoken to-night, or merely as a means of combating putrefaction. He puts forward the medicine as the best combination of sulphurous and carbolic acids he could find ; as possessing in a high degree the power of both “to extinguish the vitality of ferments,” and so as claiming to be considered the most suitable and efficient means of retarding changes of a zymotic character in the living body. I first tested it in a case of fœtid abscess of the lung, and its extraordinary effect in removing almost instantly the fœtor of the breath and expectoration, led me to try it also in typhoid fever with septic discharges, decidedly putrescent in their odour. There its effect was equally marked. I was then led to administer it in cases of scarlatina and diphtheria, and with such successful results that I have since relied on it with more confidence than on any other treatment. There is another point which I wish to remark upon with regard to this medicine, and that is the point to which Dr. Gordon alluded—its remarkable power of lowering the pulse and lowering the temperature. I may mention one very urgent case—the case of a young man to whom I was called in the stage of invasion before the eruption had become well marked, where the surface was intensely red, the temperature very high, and only a number of small spots, the size of a pin’s-head, had appeared on the body. I ordered him half drachm doses of the sulpho-carbolate of sodium every three hours. The effect produced was most remarkable in regard to the pulse, the temperature, and the urgent cerebral symptoms. The gentleman was in a state of unconsciousness, almost of complete coma, perfectly mute, and remained in that state for twenty hours. He was given a dose of the medicine every three hours. In thirty hours from the time he took the first dose his pulse had fallen from 134 to 90. I have seen the same fall of the pulse in other cases. I think it well to bring under the notice of the Society the testimony I have received to the same effect from a very intelligent physician practising at Swatow, in China, with regard to carbolic acid. “Since I wrote to you,” says Dr. Scott, “I have tried the carbolic acid in small-

pox, and with the most wonderful results. The first case was a tolerably severe one, with confluent eruption. His pulse was 140, respiration 60, temperature 102·4. I gave him five minim doses of the acid every three hours, and in six hours the pulse had fallen to 130, the temperature to 101, the respiration to 40. In twelve hours his pulse was 96, the temperature 100, and on the following morning the temperature was nearly normal, the pulse 84, and the respiration 30; and the man who on the previous day was almost in a state of coma declared himself nearly well and went on to recovery." I think that fact is worthy of being brought before the Society on account of the effect of carbolic acid with regard to the pulse and temperature being so nearly identical with that which Dr. Gordon and I have both observed and ascribed to the sulpho-carbolate. I may mention one other circumstance. I had the advantage of Dr. Gordon's assistance in a case of small-pox in which the patient was suffering from severe throat affection. Dr. Gordon advised the application of leeches at the angle of the jaws, and this was followed by great and permanent relief of the throat symptoms.

Dr. FOOT, in reply, upheld the use of the word antiseptic, as applied to the treatment of small-pox. It was a word in use wherever zymotic pathology was studied; and the phenomena of small-pox ought to explain the propriety of the adoption of this special line of treatment.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

Saturday, March 16th, 1872.

DR. KIDD, President, in the Chair.

A Case of Sudden Death, Seventeen Days after Delivery, from Embolism of the Pulmonary Artery. By JOHN RINGLAND, A.B., M.B., M.D. Univ. Dub.; M.R.I.A.; Fellow and Censor of the King and Queen's College of Physicians of Ireland; Ex-President of the Dublin Obstetrical Society; Professor of Midwifery in the Ledwich School of Medicine; Senior Master of the Coombe Lying-in Hospital, Dublin, &c., &c.

CASES of sudden death must ever present points of interest to the medical practitioner; but when such occur within a limited period after delivery, and with but little or no apparent cause, a tracing out of the actual cause will necessarily be a source of anxiety and research to her attendant, and its elucidation present features of interest to every practitioner and physiologist. I feel, therefore, that in submitting the particulars of such a case, I will not be deemed a trespasser on the time of the Society.

The subject of this memoir, who had been ten months married, was seen by me for the first time about three weeks antecedent to the date of her labour. She was 26 years of age, of a leuco-phlegmatic habit, and of a rather indolent temperament. She had enjoyed remarkably good health during her pregnancy, and presented no unusual symptom on the occasion of my visit, excepting that her pulse numbered 100; and it may not be out of place here to state that it did not deviate from that number on any of the occasions of my three subsequent visits. She required no medicine during this period, excepting an occasional aperient, and the ordinary cardiac mixture of aromatic spirits of ammonia in camphor mixture.

Her labour commenced about six o'clock on the evening of 1st of last October, but the pains were so trifling, and the intervals so lengthened, that the nursetender did not deem it necessary to summon my attendance until one o'clock on the morning of the 2nd. When I reached the house—which was some distance out of town—at half-past one o'clock, I found that the child had been just born, the patient having suffered but little during the entire labour. Her pulse was now 120, but this trifling acceleration was only what usually arises in the ordinary course of a labour, and consequently demanded no especial attention. Within a very few minutes after my arrival, the placenta and membranes—quite perfect and untorn—were expelled by the unaided action of the uterus; and nothing untoward having arisen, I was enabled to leave her without any further delay than is usual with me in perfectly natural cases—namely, two hours after the expulsion of the placenta.

On entering her room on the following morning about twelve o'clock, I was overwhelmed by the most intolerable stench, transcending in abomination any my sense of smell had ever previously encountered. Apprehensive that the nursetender—who, however, was an excellent and highly trained one, and had been named by myself—had been guilty of some neglect, and had omitted to remove from the apartment some of the soiled articles of clothing or bedding which had been used the preceding night, I made a complete search throughout the room, but found nothing to account for the occurrence, until, on raising the bed-clothes, the abominable smell became intensified, and further scrutiny demonstrated that it emanated from the lochial discharge. It had been blowing a gale of wind throughout the morning directly on the bed-room window, which had consequently to be kept closed, the door alone affording ventilation; but I now had the upper portion of the window opened, the uterus and vagina well syringed out with a solution of the permanganate of potash, and a free application of same sprinkled over the bed and bedding, and by these means the apartment was soon rendered perfectly sweet. The employment of the permanganate of potash was persisted in throughout the remainder of my attendance, although no subsequent demand for its

use presented itself, nor was any further treatment necessary, and she was convalescent and able to sit up in her room on the tenth day. During the whole of this period, however, her pulse, which had come down to 100 within twelve hours after the completion of her labour, never subsequently varied from that number. Owing to this one fact, I prolonged my attendance on her beyond the usual period, visiting her every second or third day, but still found no reason for special treatment.

Her husband having been obliged to proceed to England on business, her father and brother spent with her the evening of the 18th of October, being the sixteenth day after her delivery; they remained with her until ten o'clock, and during the whole of their visit she appeared to be in perfect health and spirits. At a little before six o'clock the following morning, when the nurse was giving her a drink, she complained of a sudden pain in the calf of her left leg, twitched convulsively at her mouth, from which some frothy saliva was expelled, and in less than ten minutes was dead. When I reached the house at about half-past six o'clock, life had been extinct for a considerable time.

I was naturally much pressed by her afflicted relatives who were in the house at the time for my opinion as to the cause of her sudden demise, and expressed my conviction that it was the result of an embolism in one of the large arteries, most probably the pulmonary; but not satisfied with mere conjecture, and hoping for an elucidation of the mystery, I applied to her husband, immediately after his return home, for permission to make a *post-mortem* examination of the body, which was at once accorded. Feeling assured that in an inquiry of the kind, the investigation of a skilled pathologist would prove the most satisfactory, I requested my friend Dr. Mapother to make the necropsy, which he most kindly did on the 20th of October, in the presence of Dr. Kidd, Dr. Hill Ringland, and myself; and I here append his report, with which he furnished me immediately afterwards.

"*Post-mortem* examination made 58 hours after death. Decomposition very far advanced. The superficial veins were marked by the oozing outwards of fluid blood; sub-cutaneous fat very abundant; right lung wholly adherent to costal pleura; tuberculosis in upper lobe; left lung also slightly adherent to ribs. Heart small, with much fat on the surface; its tissue was very soft and flabby. The pulmonary artery and its two branches were filled with a soft clot, not containing plastic fibrin. The liver and spleen were much congested; they were remarkably soft, breaking down on the least touch. The peritonæum was quite healthy. The cavity of the uterus was about as large as to contain a closed hand, but its walls were as thin as those of the bladder. The organs of circulation and respiration were evidently in a state most highly favourable to death by syncope."

This case presents several points meriting attention, of which not the least remarkable was the persistent frequency of the pulse. No doubt a medical attendant must ever feel anxious so long as the circulation continues excessive after the completion of labour, a point of no small importance, to which Dr. McClinton drew the attention of the Society some time since; but the existence of that condition antecedent to labour on each and every one of my four visits spread over a period of three weeks, and its unaltered state during many days subsequent thereto led me to the conclusion that such was the normal frequency of her pulse, the more especially as its character in all other respects was natural, and consequently after the first few days it did not appear to demand special attention.

Again, the enormous amount of disease which the autopsy demonstrated to exist in almost every organ of the body, and which yet during life was indicated by no symptom whatever—unless the rapid circulation be deemed such—must necessarily amaze us. How the vital functions in her case were carried on at all with a heart a mass of fat—the lungs adherent and filled with tubercle—the liver enlarged with fatty deposit, and breaking down on the least pressure of the finger—and the spleen soft and congested—is a marvel; and although this great amount of disease could not be of recent existence, still she never complained to any member of her family of a single symptom indicative of the existence of any one of the disorganizations subsequently found.

No doubt the condition of the heart and lungs, as described in Dr. Mapother's report, culminated in the formation of the clot discovered at the bifurcation of the pulmonary artery; and although we found this clot soft, no doubt but its softened condition resulted from the frightful decomposition which had taken place in all the tissues, and in all probability the existence of the clot was the *immediate* cause of the suddenness of her death.

The most interesting point in this case to the practical obstetrician was, to my mind, the extreme thinness of the uterine wall, which, despite the enormous amount of degeneration which was found to exist, still performed its part without yielding; and I have more than once congratulated myself that no delay nor obstacle arose in the progress of her labour demanding interference, and that no manual removal of the placenta was required, as I feel assured that had any such contingency arisen, rupture of the organ must have ensued, in which case undoubtedly the odium of the casualty would have been undeservedly cast on the necessary interference, and not on the real cause—the debility and disorganization of the organ.

Before concluding, it may not be out of place to state that the *post-mortem* examination afforded no explanation of the cause of the remarkably offensive lochial discharge.

The PRESIDENT said that no position in which a medical man could be placed was more serious or important than that described by Dr. Ringland. He had had an opportunity of seeing the *post-mortem* examination, and he was greatly struck with all the points Dr. Mapother had alluded to in his report. There was very rapid decomposition, for which the warmth of the weather was not sufficient to account. The whole body was bloated; every superficial vein was green and mapped out upon the surface, and the odour most offensive. The thinness of the uterus was a most remarkable point, and showed how easily the uterus might sometimes be ruptured, even in the most carefully conducted operations. Dr. Playfair made an interesting remark in connexion with embolism. He said that true embolism never occurred before the 19th day after delivery; that in the cases he had examined the earliest period at which a clot had become loosened so as to allow of its being carried into the current of the circulation was the 19th day. The difference between an embolus and a thrombus must be observed—a thrombus being a clot formed in some blood-vessel and obstructing the circulation at that point; an embolus a clot or thrombus which becomes detached from where it was originally formed, and is carried elsewhere in the course of the circulation till its progress is stopped, it may be, in the pulmonary artery. Dr. Playfair has found no case in which this loosening, detachment, and floating off of a clot, occurred earlier than the 19th day; but a thrombus forms at a much earlier period, and Dr. Ringland's case might be one of thrombus. During pregnancy there was an excess of fibrin in the blood, a condition likely to favour the formation of a thrombus, and if they added to that a weak heart and a diseased condition of the lungs, which made an additional call upon the heart, they would see there were sufficient reasons to account for the formation of a clot.

Dr. CHURCHILL thought they were under great obligations to Dr. Ringland for bringing this case forward, for it was not always they could get at the anatomical cause of death in these cases. In private practice one could rarely get a *post-mortem* examination, and therefore every case in which an autopsy was allowed should be taken advantage of and recorded. He had accumulated a good many of these cases in a book which he had published. He had only seen two cases in private that could come fairly under the same category as that described by Dr. Ringland, and in neither of these could he tell the actual cause of death, for no *post-mortem* was allowed. The cases were excessively distressing and seemed obscure. One of them admitted of the explanation given by Dr. Ringland. They all knew that an eminent lady, the Duchess de Nemours, died on the 14th day from embolus of the pulmonary artery. One of the two cases to which he referred was a lady whom he attended

at the delivery of her fourth child. There was a delay in the second stage, but with the least assistance by the forceps she was delivered, and the child was now alive. She recovered without a single bad symptom, and on the 11th day he paid her an extra visit to see that all was right. He saw her in bed, found there was no tenderness on pressure, and she said she was as well as ever she was in her life. The next day she was getting up to go down to her drawing-room; she had nearly dressed herself, and had put on her ear-rings, for she was going down in state, when she had a call to pass water, and sat down, but immediately gave a loud cry and fell down dead. No doubt death in that case resulted from embolism. The second case required a different explanation, though similar to the other in one respect—that both were delivered by the forceps. It was her first child; the case presented no difficulty whatever, and he left the lady in an hour and a half with a quiet pulse and in every way doing well. Within an hour afterwards he was summoned back in a hurry. He took it for granted that it was a case of hæmorrhage, but when he got to the house he found the lady not blanched, but with a livid tint of face, rather purple lips, and in the act of dying. His first impression was that there had been large hæmorrhage, and he not only asked the nurse but took care to examine the bedding, and there was none. Then came the question, what did she die of? He believed she died of that disease sometimes called syncope, sometimes idiopathic asphyxia, or, as he thought more correctly, cardiac paralysis. There was no hæmorrhage, and no time for clots to form. He suspected a form of heart disease played a more important part in these cases of sudden death than was yet acknowledged, and he thought it had much to do with the fatal result in Dr. Ringland's case. When there was a weak heart a little extra work would often bring it to a stand-still.

Dr. HENRY KENNEDY entirely concurred in what had fallen from Dr. Churchill. He believed that death had resulted from fatty heart, the symptoms being such as would accompany that affection, whereas they were not those of embolism. The person's temperament and constitution, the very fact of the discharges being unwholesome and foetid from the commencement, showed a state connected with fatty heart. He had recorded many observations in connexion with frequency of pulse or otherwise in cases that terminated fatally. In many of these cases the pulse was remarkably slow, falling to 40, 30, and 26. In another class the pulse got quick at a certain period, and so remained up to the death of the individual, and this was a state which might go on for weeks. An extraordinary case, bearing on this subject, occurred some years ago in Dublin. A gentleman had been treated in Limerick for gall stones, and got an enormous quantity of opium; he recovered

from a state of partial narcotism, and came up to Dublin, where he was visited for several days by Sir Henry Marsh and other medical men after the effects of the opium had completely passed off. During all that time nothing was observed about the case but a very rapid pulse. The gentleman died, and on a *post-mortem* examination being made, nothing was found but a fatty heart. There were certain cases in which a fatty heart was known by a rapid pulse. Decomposition always set in very rapidly in these cases, and the condition of the veins, such as existed in Dr. Ringland's case, arose from their containing air. One other remark he wished to make, namely, that it was fortunate there had been no occasion to use chloroform in this case. He believed that in consequence of the fatty heart the patient would have died under its use.

Dr LALOR asked if Dr. Kennedy had known of any case of a fatty heart in which the pulse continued at 100 or 120 for weeks before death.

Dr. KENNEDY replied in the affirmative.

Dr. ATTHILL said, that in listening to the paper, the idea struck him, which Dr. Henry Kennedy had illustrated, that this lady was suffering from a fatty heart, but he thought that that condition was not the actual cause of death. The case was, in his opinion, one of a class of which he believed he had seen two others, where the feebleness of circulation was such as to permit a clot to form which would not have been formed in a perfectly healthy person. It was not a case of true embolism. The clot formed by embolism was entirely different from the soft clot which Dr. Ringland had described; an embolic clot was firm, and contained a nucleus of firm old fibrine, showing well-marked concentric layers. In Dr. Ringland's case, he fancied that from the heart's being feeble a coagulum formed in one of the vessels, and so caused death. In alluding to Dr. Ringland's case, he had before his mind an instance which he had seen some months ago. The subject was an apparently healthy lady of a full habit, but of leuco-phlegmatic temperament. She made a good recovery after a rapid labour. Eight days after delivery she took a saline purgative, and got out of bed to go to the night-chair. On standing up to get into bed again she fell back fainting. She had just power to ring the bell, and when her attendant arrived she was in a state of extreme collapse. Her extremities were cold, respiration was hurried and laboured, consciousness was perfect. Hot water jars were placed to her feet, and she rallied. He did not see her until twelve hours after the attack. Warmth had then been restored, but her pulse was so small and rapid that he could not say what its actual rate was, but it was not under 130; the heart's action was strong. He expressed the opinion that a clot had formed in the pulmonary artery, and he treated her by

the exhibition of large quantities of ammonia. In eight days more she ventured to raise herself in bed contrary to advice, and fainted. She had since gradually improved, and was now tolerably well. He believed this lady had a feeble, if not a fatty heart. He thought the syncope had been occasioned by a clot, which had, under treatment, gradually dissolved. He saw, last year, a somewhat similar case occurring in a patient twenty-one days after labour. She had got down to her drawing-room, and having used some exertion, fainted; the symptoms which followed were the same as in the other case:—coldness of the extremities, rapid respiration, a very feeble and rapid pulse, and strong heart's action. He believed that in this case also a clot had formed. This patient also recovered. No symptom of valvular disease existed in either of these two patients, and those which presented themselves could, in his opinion, be only accounted for on the supposition that a clot had formed.

Dr. RINGLAND, in reply, said his paper had elicited a most interesting discussion, and there was in reality no difference of opinion on the case. Some thought the cause of death was debility of the heart; he thought it embolism, but a weak heart was certainly the first cause; and, therefore, if they could be said to differ at all, at least their differences would be found to harmonize. To him the condition of the uterus was the most interesting feature in the case. When taken from the body it was like a moderately distended bladder. When Dr. Mapother held it up between him and the light they could see his fingers through it as if it were a piece of red-coloured glass. The wall of it was scarcely thicker than ordinary brown paper. He had assisted at many *post-mortem* examinations, but he had never seen a uterus in that condition before.

Dr. TAYLOR described a case of cross-birth, in which, after considerable difficulty, he succeeded, by means of a hook passed under the axilla, in delivering a woman of a child which had been dead when he was first called in. The weight of the child was 5 lbs.

On Ergot of Rye. By JOHN DENHAM, M.D., F.R.C.S.I.; Ex-Master, Rotunda Lying-in Hospital.

SOME sixteen years ago I published a few observations on the use of ergot, in *The Dublin Quarterly Journal*, the principal object of which was to show that the injury inflicted on the child in utero by ergot was mechanical, not poisonous. We have long been familiar with the statement that the continued use of ergot of rye acts as a poison on the animal economy; but, as Phœbus observes, we cannot call it a violent poison, since drachms, and even ounces, are required to destroy small animals, such as dogs, rabbits, and pigeons. If we call to mind the experiments of Magendie on such animals, by which it is clearly proved that

when they are fed exclusively upon any one substance, such as white sugar, white bread, corn, hay, or barley, although they appear to thrive for the first week, yet they invariably die at the end of a month or six weeks. If we compare the symptoms produced in dogs compelled to swallow ergot (for they show great aversion to it), with those the result of exclusive feeding upon one particular substance, we will be greatly struck with their similarity. On the same principle, I think, that the ergotism so often met with in some of the Provinces of France and other parts of the Continent of Europe, from the use of diseased grain and damaged wheat, might, to a large extent, be accounted for by the absence of those nutritious particles, a combination of which are found to be indispensable to life, rather than to the fact that ergotized grain is directly and decidedly a poisonous substance. That ergot of rye is by no means the deleterious poison that some would lead us to believe, is forcibly shown by the experiments of Black. He states that he gave 9 lbs. of ergot daily to 20 sheep without any ill effect. 20 other sheep consumed $13\frac{1}{2}$ lbs. of ergot daily for two months without any bad effect; 30 cows took together 27 lbs. of ergot daily for three months with impunity.

Many of the experiments of Wright (see his Prize Essay on Ergot, 53rd vol. of *Edinburgh Medical and Surgical Journal*), tend to the same view, although he is largely quoted by some authors, in proof of the direct and immediate poisonous nature of ergot on animal life. In the space of 12 weeks he gave to a large doe rabbit 64 ounces of solid ergot; at the end of the 12th week she died; but what is most remarkable, during the 12 weeks she brought forth young twice; six on the first occasion, all of them lively, of good size, and apparently in good health; on the second occasion she gave birth to three young ones, two of which were dead, and not more than half grown, the third alive, but it died in a few hours. Toward the close of this last period the poison had evidently begun to tell upon the rabbit; she looked ill, appeared to be drowsy and moping, and her hair became long, erect, and rough, and she exceeded her previous period of gestation by five days.

My object, however, on the present occasion, is not to discuss the subject of ergot in its physiological aspect, but rather to bring under the notice of the Society the result of my observations in a practical point of view.

We have given it again and again in the chronic wards of the Rotunda Lying-in Hospital, as an emenagogue, but with no beneficial result worthy of being recorded. Given in small and oft-repeated doses, it sometimes seemed to do good in cases of leucorea, but the free internal use of iron with a generous diet and astringent injections, are much more to be relied upon.

In some forms of hæmorrhage, such as we often meet with, some days

or even weeks after delivery, where the uterus remains softer and larger than usual, we have found it beneficial; and in not a few where the hæmorrhage was caused by an internal polypus, the ergot seemed to hasten the expulsion of the polypus, but in cases of hæmorrhage the result of fibroid enlargement, either on the internal or external wall of the uterus we found no benefit whatever from the use of ergot. This I saw exemplified fully, within the last two months, in the case of a lady who was suffering from prolonged and repeated hæmorrhages caused by fibroid enlargement of the entire uterus. I saw her, for the first time, in consultation with a relative of her own, a practitioner from the country; he was not, however, her regular medical attendant. The tumour was of some years standing, and the hæmorrhages had lately become much more severe and more prolonged. She was greatly emaciated, and her face presented a death-like pallor, with a considerable amount of œdema about the upper and lower eyelids; she suffered greatly from palpitations and noise in the head. I proposed the immediate use of the solid nitrate of silver, which we had found to act most favourably on cases of hæmorrhage in the hospital, that had resisted every other form of treatment. The lady's friend, whom I met in consultation, would not consent to such heroic treatment, and pressed so hard for a trial of ergot, which we found had not been previously given, that it was ordered as fully and freely as the patient could bear it. The preparation used was the liquor ergoti, as prepared by Hamilton and Long, of this city, a medicine I can speak of with the greatest confidence, from my experience of it in other cases. In the present case, however, it was wholly powerless for good, and seemed rather to increase the hæmorrhage than the reverse. The napkins worn by the patient were saturated with a pale watery discharge, but whenever she had occasion to rise numerous large soft clots escaped from the vagina.

The nitrate of silver was now tried. I succeeded with not much trouble in passing up from a quarter to half an inch of the solid stick of the nitrate of silver through the os and cervix into the cavity of the uterus, where it was left. She complained of a burning pain in the part for some time, which was relieved by an anodyne, but in no other way did she suffer; the hæmorrhage which had been going on for four weeks continuously now entirely ceased, and did not return until her next menstrual period, before which time some colour had returned to her cheek, and her general health had so much improved that she was able to take carriage exercise, and has now gone to the country; but I dare not say, or even hope, that the hæmorrhage will not return.

The object to which I particularly directed attention in my former paper on this subject was, how and to what extent ergot affected the child in utero. To that subject I again beg leave to call the attention of the present meeting.

Dr. Beatty, my esteemed friend, Dr. F. H. Ramsbotham, and Drs. Hardy, and M'Clintock, all maintain that the poisonous effect of ergot is extended from the mother to the child. Dr. Hosack and Dr. Meigs of Philadelphia support the opinion that death is produced by the violent contractions of the uterus upon the child, and the compression and contortion of the uterine vessels consequent thereon. Dr. Chapman states that in 200 cases in which ergot was given it produced no harm whatever. No one here, he adds, believes in the alleged deleterious influence of the article upon the fœtus.

In Dr. Beatty's very valuable and truly practical work, *Contributions to Medicine and Midwifery*, he describes six cases of poisoning by ergot, and says of them:—"In the cases just recorded the condition of the infants was very unlike that of still-born children delivered under ordinary circumstances, and when no ergot had been administered to the mother. The distinguishing characteristics are the general lividity of the surface, the universal rigidity of the muscular system, producing the stiffened limbs and clenched hands in those infants in whom life was extinguished, and the remarkable kind of alternating spasms and palsy which supervened in those that were resuscitated." Again, at page 160, it is stated—"From these observations I think we are justified in coming to the conclusion that the administration of ergot of rye to a woman in labour is attended with danger to the child, whenever a time sufficient for the absorption and transmission of its noxious properties elapses before the child is born, and from the cases above stated I am inclined to place two hours as the limit of safety, and to consider a prolongation of labour beyond that period as perilous to the infant." In the late valuable report of the Rotunda Lying-in Hospital, published by Dr. Hardy and Dr. M'Clintock, they remark, in speaking of the effects of ergot in tedious labours, page 84:—"It by no means follows as a consequence that the ergot will not act on the child because it does not act on the uterus, for we have seen numerous instances where the child was unquestionably affected by it although the uterus was wholly unaffected or nearly so." At page 95 they remark:—"The total number of tedious and difficult labours in this report amounts to 259; of this number thirty got ergot to overcome inertia in the second stage of labour, and only 10 out of the 30 children were born alive. This furnishes strong proof, they say, were any such required, of the deleterious influence of ergot upon the fœtus, as in nearly every one of them there was unequivocal evidence of the child's vitality where the ergot was given, and in the great majority of them delivery took place within two or three hours after the administration of the medicine. The proportion of deaths recorded by Drs. Hardy and M'Clintock where ergot had been given is unquestionably great—10 children only out of 30 being born alive. May we not fairly surmise that death would have occurred in many of those cases even if

ergot had not been given? I am the more impressed with this idea by finding in their report mention of several cases of tedious labours where ergot had *not* been administered, but in which the children were still-born. Thus, in Case 39, they state:—A very short time before the birth of the child the foetal heart was audible; nevertheless it was still-born and could not be animated. No. 98:—Delivery was effected by the natural efforts at 4 o'clock; the child, a boy, was born dead, notwithstanding that the pulsations of its heart had been heard at 2 o'clock in the day. And in Case 211 attention is directed to the important fact of the child's being dead-born, though its vitality shortly before the patient got the bath had been satisfactorily ascertained. Need I say how often we all meet with cases of still-born children where ergot has not been administered, and in which we had no reason to apprehend such a result.

In a separate paper by Dr. Hardy we have the results of 48 ergot cases, in 34 of which the children were dead-born, yet in none of them did he witness the symptoms described by Dr. Beatty—at least he makes no mention of them—and we may reasonably infer that so diligent an observer would not have passed over appearances so unusual and remarkable.

In the 30 cases recorded by Drs. Hardy and M'Clintock, 20 of which were fatal, there is no mention made of any such symptoms as the result of ergot.

I beg leave to notice, as bearing upon this subject, a very able paper on Impending Dissolution and Nervous Affections in Young Infants, published in 25th Vol. of *Dublin Quarterly Journal*, by Dr. Doherty, Professor of Midwifery in Queen's College, Galway. He there gives a detail of upwards of 20 cases, in all of which there was present at or very soon after birth a tendency more or less to convulsions, spasmodic twitchings, or paralysis. Many of the children were born cold, livid, and congested, while some were pale and flaccid. In Dr. Collins's report of the Dublin Lying-in Hospital, he states that of the 16,654 children born in the Hospital during his Mastership, 1,121 were still-born. Of those born alive, 284 died previous to the mothers leaving the hospital; of these, however, 100 were premature, 32 children, born at the full period, died a few minutes after birth, and 70 other children born at the full time died in periods varying from half-an-hour to several days. In these 70 cases, Dr. Collins remarks, the labours were in almost *all* of a very short duration; yet above one half of the children died within twenty-four hours from birth.

The word ergot is not so much as named in the entire report, nor do I believe that Dr. Collins ever gave a single grain of it to a patient during his Mastership in the hospital; from which we may safely infer that children die, even without the aid of ergot, both before and after birth. It is somewhat remarkable that both those who look upon ergot

as a poison, injurious alike to mother and child, and those again who maintain that it is wholly innocuous, at least in the doses usually administered, all appeal to the experiments of Wright already alluded to. Permit me to give his opinion in his own words. "I have never," he says, "seen a case justifying the conclusion that mischief has followed the cautious employment of ergot, nor should I, from my own experience, consider it a medicine, the exhibition of which, in a judicious manner, would be at all likely to injure either mother or offspring. Its tendency to injure the child when given during the parturient stage only I can scarcely believe, unless the impression be mechanical, and the foetus be hurt from the contractions of the uterus upon it, or from a disproportion between the external parts and the foetus, the head suffers from being impacted in the pelvis. But if freely given for some time prior to the period of delivery, I am fully inclined to the opinion that it may, by deteriorating the health of the patient, so communicate its influence to the offspring as literally to destroy life or materially to reduce it in strength or soundness."

For some time past we have been engaged in observing the effect produced by ergot during the period of utero-gestation; and we have had forced upon us the conviction that ergot if given say between the sixth and ninth month, it neither affects the life or health of mother or child, nor does it bring on labour until the full period of utero-gestation is completed. Permit me to give the details of one of the cases alluded to:—

Jane Doyle, a fine healthy young woman, from the country, unmarried, and, according to her own account, not pregnant. Was admitted into the chronic ward in May last. The foetal heart was distinctly audible. On 18th, 21st, and 23rd of May, she got 20 gr. of freshly powdered ergot three times each day without producing the slightest perceptible effect. Early in June we gave her $\mathfrak{z}\text{i}$. of liquor ergoti three times a day for seven days almost consecutively. It produced no effect whatever, except to sicken her very much, until at length she refused to take it any longer. She remained in hospital nevertheless, and was delivered on 26th July of a healthy male child, with nothing whatever remarkable in its appearance. This woman took $12\frac{1}{2}$ drachms of ergot.

It frequently happens that women come into hospital suffering from spurious pains, but not in labour, although they have reached their full time. In such cases we have often remarked that the ergot, if persevered in for a day or two, generally brought on the labour. For example, Mary Wickham, aged twenty-nine, admitted on her second child, on 25th June last. Found not in labour. She was ordered three draughts, containing $\mathfrak{z}\text{ii}$. of liquor ergoti. On 26th the ergot seemed to have produced no effect, and the draughts were ordered to be repeated every fourth hour. Her labour came on in the course of this day, and she was delivered of a fine healthy child at 9 50, having taken in the two days an

ounce and a-half of liquor ergoti. We do not believe that the giving of ergots will bring on abortion in healthy women, even where there has been some threatening of it; but if the process has really set in, then the ergot rapidly hastens it. In numerous instances of threatened abortion we have given it with the hope of either arresting the hæmorrhage or bringing on the abortion, but apparently without any effect, as the hæmorrhage has gradually ceased, although we could not attribute that to the ergot, and the women have often gone on to the full period and given birth to healthy children. Our experience of ergot has not, I regret to say, increased our confidence in it as a remedial agent in *post-partum* hæmorrhage, either before or after the expulsion of the placenta. Before the removal, because it induces a condition of the uterus unfavourable to the introduction of the hand should that operation be found necessary. After the removal of the placenta, because of its depressing effect on the circulation and nervous system, and because it very often induces nausea and vomiting, but especially because we have not found it to produce its specific effect on the uterus after it has been entirely emptied of its contents; and further, because we almost invariably find that steady pressure on the fundus with the hand, and the free application of cold water, both externally, and more especially the direct introduction of it into the vagina and uterus by means of a gum-elastic tube more prompt and efficacious than all other remedies. Where from the previous history of the patient we have reason to apprehend *post-partum* hæmorrhage, we never fail to give ergot towards the end of the second stage of labour, and with the happiest effect. To Dr. Beatty we are indebted for this valuable suggestion in the treatment of hæmorrhage, and it is one of great practical importance.

In cases of tedious labour even where the delay arises from simple inertia, and in which the ordinary remedies, such as change of position, stimulating injections, &c., fail, we do not so often give ergot as we formerly did, but prefer using the forceps, by which we believe the mortality of both mothers and children has been considerably diminished. Our experience of ergot during the period of pregnancy has led us to the following conclusions:—1st. When given to a pregnant woman, even in repeated doses, at any other than the full period, it produces no effect whatever beyond nausea and loss of appetite. 2nd. It produces no injurious effect upon the fœtus in utero. 3rd. Where the process of abortion has commenced, the ergot hastens it materially, and acts very beneficially. 4th. When given in the second stage of labour, it often acts beneficially and hastens the labour, but if the labour be not soon completed, it is attended with great danger to the child, not from any toxic effect of the ergot, but by the mechanical obstruction it offers to the circulation, and the continuous and powerful contraction exerted on the child.

In *post-partum* hæmorrhage after the expulsion of the placenta we do not believe it exerts much influence on the uterus, therefore we seldom use it in such cases; but rather trust to other remedies already alluded to.

There are few countries in which charms or medicinal agencies do not and have not existed for the purpose of inducing labour or increasing uterine action. For example—in a work lately published by a Mr. Whimper, on British Columbia and Vancouver's Island, he mentions that the tail or rattle of the rattle-snake in powders is used as an infallible remedy for inducing or increasing uterine action. And in our own country, Erin's green isle, the shaking of a labour-patient, or waving her backwards and forwards nine times, and then giving to her nine grains of blasted corn or grain, was looked upon as a certain means of bringing on or increasing uterine pains.

It is possible, perhaps probable, that our American friends who claim credit for the discovery of ergot as a therapeutic agent in the practice of midwifery, are indebted to some wise old Irish crone, who emigrated to that land before many, now present, were born, for the hint which led to the discovery of this medicine now so generally used, and, I may add, often abused.

I beg leave, however, to draw the especial attention of the meeting to the varied uses to which this medicine has lately been applied, and the high repute it has obtained as a powerful remedial agent in arresting hæmorrhage, and that speedily in almost every organ of the body.

Dr. Alfred Meadows, in a paper published in the *Practitioner* for September, 1868, urges the extended use of ergot in variolus uterine and vesical affections. He has clearly demonstrated by experiment that ergot contracts the minute arteries.

Thus if a single grain of ergot in solution be injected sub-cutaneously into the web of a frog's foot, in a few minutes the convulsion will be seen to become much quicker, then it will stop for a few seconds and oscillate in a spasmodic jerking manner. In about half-an-hour the blood current will gradually return to its normal even flow.

The well known experiments of Brown-Sequard, give further evidence on this subject. He found that the vessels of the pia mater in the dog contracted during the administration of ergot just as with belladonna, but that ergot acted on the cord the more powerful of the two.

The experiments of Dr. Ch. L. Holmes, not only show that ergot contracts the minute vessels, but also that the blood pressure is not raised. He found that the injection of ergot into the jugular vein of a dog was followed with great rapidity by sudden diminution of the blood pressure, which remained so for several seconds, and then reached a higher level than before the injection.

Ergot has long had the reputation of being a styptic. Dr. Dobell, recommends it, but gives it in combination with digitalis gallic acid and

other preparations, but by thus dividing the honours he leaves it doubtful which of the remedies really produced the desired effect. Dr. Harman Weber also speaks favourably of ergot as a valuable agent in arresting hæmorrhage in severe cases of hæmoptysis.

We have an interesting paper by Von Langenbeck, detailing the successful treatment of two cases of aneurism by the hypodermic injection of ergot.

Dr. Dutoit, of Bern, and others, have reported cases of aneurism successfully treated by this method. In the *Edinburgh Medical Journal* for July, 1870, we have recorded a very severe case of hæmoptysis completely checked in three minutes by the subcutaneous injection of five grains of ergotin. Dr. Allen Jameson records a similar case only not so severe, successfully treated by the same means; and lastly, we have a very interesting paper in the *Practitioner* for December, 1871, by Dr. C. Currie Ritchie, of Manchester, giving a detailed account of eight cases of hæmoptysis, in all of which the hæmorrhage was rapidly and in most of the cases permanently checked, by the hypodermic injection of ergotine, in five grain doses. In the same paper, he also mentions another case which he saw with his colleague Dr. Bornman; the patient aged sixty, was expectorating blood profusely, an incessant hacking cough impelling her to do so every few seconds; five grains of ergotine were injected, after which she had absolutely no hæmoptysis.

A case of hæmatemesis, is also given by C. Stuart, M.B., in the *Edinburgh Medical Journal* for December, 1871, which was successfully treated by a single hypodermic injection of three grains, after other powerful remedies had failed.

If we can bring ourselves to believe that a single hypodermic injection of 5 grs. of ergotin can almost instantaneously put a stop to bleeding in severe cases of hæmoptysis, epistaxis, or hæmatimisis; surely we are called upon to give it a trial in uterine hæmorrhage, the most frequent and the most fatal of all hæmorrhages. I trust that the masters of our lying-in hospitals, if they have not already begun, will soon make application of this remedy, and give to this Society the result of their experience.

Dr. JOHNSTON said he believed ergot did not produce a poisonous effect on the mother or on the foetus. When he was an assistant in the hospital he investigated this subject very fully, and the practice then being was to wait much longer than at present and direct the use of ergot. In the account which he published of his observations, he stated that as many as eight hours elapsed from the first dose of ergot to the child's birth, and in the greater number of these cases the child was born alive. Now-a-days, thanks to the improvements made in obstetrics, they did not depend on ergot for the delivery of the child, but merely used it in order to

secure contraction of the uterus in cases where they were obliged to administer chloroform. They had now the courage to use the forceps more frequently than in former years, and the use of ergot to promote delivery was therefore not required.

Dr. RINGLAND observed that the paper was a valuable one, and the Society could not feel too much indebted to Dr. Denham for opening a discussion on this important question. He fully agreed with the views of Dr. Denham, that the idea of any poisonous effect being attributable to this drug might be entirely discarded. It was at a very early period in his (Dr. Ringland's) own career that the idea struck him that ergot proved fatal to the children by its action on the uterus. He recollected a very crude paper he had submitted to this Society, when a student, on cases of contracted pelvis, in which he expressed this opinion. He alluded in it to two cases in which ergot was used by Dr. Evory Kennedy. In both the foetal heart was audible before the administration of the drug. Delivery was completed in one case within seven or eight minutes, and in the other within fifteen or sixteen minutes after its employment; and although the foetal heart was distinctly audible antecedent to the use of the drug, both children were stillborn. He remarked that the action of the uterus appeared to be continuous from the moment the drug was exhibited to the completion of delivery. He did not see the cause and effect then, but they had been fully worked out by others since. He rather dreaded the employment of the drug antecedent to the second stage of labour. He had employed it in a very few cases where the head was on the perinæum and there was delay from inertia; but he thought the best ergot they could employ was the forceps. In the very few cases in which he had employed it in the second stage, the moment he saw any attempt at continuous action of the uterus he applied the forceps and delivered the child. But in that stage of labour the employment of ergot was of vast practical advantage in those cases where they had a hæmorrhagic history of the patient, and where they might expect as soon as the child was delivered there would be a dash of blood. The administration of a good dose before the child was delivered might induce uterine action which would prevent the hæmorrhage. As to its employment in the third stage of labour, he had more confidence in its efficacy than Dr. Denham. He had employed it again and again using the excellent preparation of Hamilton and Long, and it had rarely failed. He had, however, never depended on any one means alone in cases of severe hæmorrhage. It was difficult to say whether it was the handling of the uterus, the grasping of the fundus, the application of cold water, or the use of ergot that produced the effect, and he therefore recommended the combination of these several modes of treatment whenever they had to deal with any of those

fearful cases of hæmorrhage. Dr. Denham thought ergot had no effect on the uterus between the sixth and ninth month unless labour pains had been established. His own experience did not bear out this view. He had been obliged to use ergot in some cases of hæmorrhage, and found that labour was the result. Dr. Denham had recommended the Masters of Lying-in Hospitals to try the effect of sub-cutaneous injection of ergotine, and he begged to say that it was intended to give it a full and fair trial at the Coombe Hospital.

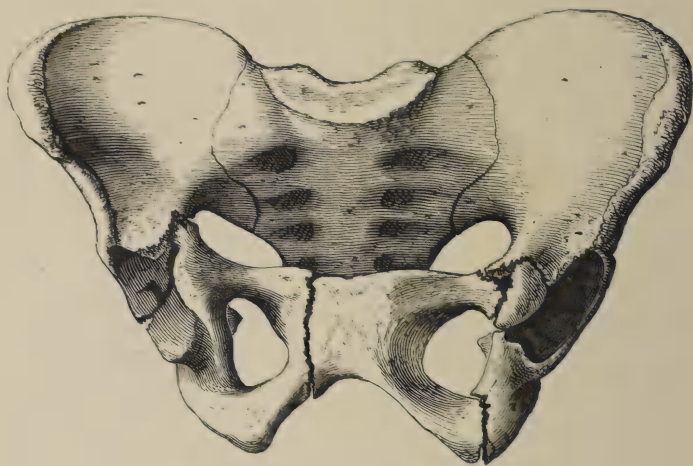
Dr. ARTHUR RINGLAND said that on a recent occasion he injected four grains of ergotine, and in four and a half minutes it brought on uterine action. It first caused nausea and vomiting, and the woman complained of this for half an hour afterwards. She had also a very sore arm, due no doubt to the spirit.

Dr. ATTHILL observed that in all cases of hypodermic injection of ergotine there was a great liability to sores forming, and that this was a real objection to its use.

Dr. BYRNE said that in many works ergot was strongly recommended for bringing on premature labour, and Dr. Denham's paper was therefore important as showing the specific effects of the drug. He agreed with those who thought that unless they were prepared to use the forceps the action of ergot was rather dangerous to the child—not he believed from any toxic effect, but from pressure upon the funis. He attended some time ago a case of twins. The first child was delivered without difficulty; there was some delay, and he administered an enema of ergot, the second child being then living; when delivered it barely breathed, and had a peculiar livid look as if it had suffered from pressure. He would not recommend ergot in the second stage to expedite labour except where the head was on the perinæum, and where the non-expulsion of the child was due to pure inertia. As to its administration between the second and third stage, he had seen cases where hæmorrhage was prevented by strong doses of ergot.

On the motion of Dr. MORE MADDEN, seconded by Dr. GUINNESS BEATTY, the discussion was adjourned.





DR. R. W. SMITH—FRACTURE OF THE PELVIS.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

Comminuted Fracture of the Pelvis ; Wound of the Bladder.—Professor R. W. SMITH laid before the members a specimen of fracture of the pelvis, and gave the following history of the case. A woman, aged sixty, was admitted into Sir Patrick Dun's Hospital, April 12, 1870, having a short time previously been knocked down by a dray, one of the wheels of which passed over her pelvis. When admitted she was much collapsed, and had evidently sustained some very severe injury. In the course of the evening, she expressed herself as being unable to pass water, and before morning it was necessary to use the catheter more than once.

When I saw her early on the morning of the 13th, all signs of collapse had passed away; her pulse was quick, but weak; she was feverish, and complained much of pain in the lower part of the right iliac fossa, near Poupart's ligament, where there was also extreme tenderness on pressure, and a slight degree of fulness. The least motion caused so much distress, that an accurate examination could not be made, but there was strong reason to believe that some part of the bony fabric of the pelvis was broken. The retention of urine continued for several days, and it was necessary to use the catheter at regular intervals. The urine drawn off was coloured with blood, leading to the supposition that the bladder had been wounded.

On the 14th and 15th, the fulness in the iliac region continued to increase, the tenderness on pressure became greater, and the skin assumed a dusky, livid hue; the swelling was tympanitic on percussion, and the crepitation of air could be felt throughout it. She had copious perspiration, which continued throughout the whole progress of the case.

On the 16th, a puncture was cautiously made into the tumour, as its tympanitic condition rendered its exact nature somewhat obscure. There escaped from the opening a mixture of air and offensive purulent matter, mixed with a fluid which, upon examination, proved to be urine. She died in a state of great exhaustion on the 26th, having survived the receipt of the injury for fourteen days. She had no symptoms of peritonitis at any period of her illness. The diagnosis arrived at was, that the pelvis was fractured and the bladder wounded. This opinion was founded upon the nature of the accident, the intolerable pain when any attempt was made to move the pelvis, and the presence of blood in the urine.

Autopsy.—The pelvis was found broken upon both sides; upon the right side (that over which the wheel had directly passed), a fracture traversed the horizontal branch of the pubis, passed vertically through the centre of the acetabulum and terminated just behind the tuber ischii; it was bisected by another, which terminated at the centre of the outer border of the articular cavity, which was broken into three portions. Another line of fracture, beginning at the spine of the pubis, traversed the entire of the descending ramus of the pubis vertically; its course was parallel and less than half an inch external to the symphysis pubis.

Upon the left side there existed a comminuted fracture of the pubis, implicating the acetabulum, and also a simple fracture of the ramus of the ischium, close to its junction with the tuberosity of that bone.

There was a lacerated wound of the neck of the bladder, to the right of the orifice of the urethra. There were no signs of peritonitis, nor any urinary infiltration in the pelvis.—*January 28, 1871.*

Aneurism of the Pulmonary Artery.—DR. BENNETT exhibited a specimen of aneurism of a branch of the pulmonary artery which had formed in the wall of a phthisical cavity, and which had caused death by its rupture.

The patient from whom the specimen was obtained had been a very short time in Sir P. Dun's Hospital during last summer. He was about 30 years of age, and was admitted suffering from phthisis. He presented the usual physical signs of a cavity in the apex of the left lung. Before any details of the history of the case were taken, he was seized, while walking up stairs, with violent hæmoptysis, and died in a few minutes. The body was fairly nourished, and all the viscera except the lungs appeared healthy.

The pleuræ, especially the left, were adherent; but the adhesions were not of recent date. Tubercular deposit extended throughout the tissue of both lungs, but the disease was more advanced in the left. Much difficulty was found in removing the upper lobe of the left lung, as its adhesion to the chest wall was most firm; in the process a large cavity was opened, occupying the greater part of its upper and posterior part. This cavity was full of coagulated blood, which extended into the bronchi and trachea; some frothy blood was present in the right bronchus, but it was clear that the source of the hæmorrhage was the left lung.

On examination of the walls of the cavity, a small globular tumour, about the size of a hazel nut, was found projecting into the cavity, which attracted attention by its dull yellow colour, and by its shape; from the side of this, close to its most prominent part, a small shred of coagulated blood hung, and served to point out the position of a small rent in the



DR. BENNETT—ANEURISM OF PULMONARY ARTERY.



DR. BENNETT—TUBERCULAR DISEASE OF THE FEMUR.

tumour leading into its interior. This tumour, which is represented of its full size in the accompanying plate, proved on careful examination to be an aneurism of one of the smaller branches of the pulmonary artery close to the root of the lung. The figure represents one of the chief branches of the pulmonary vessel, and the origin of the branch on which the tumour was seated, laid open down to the sac; a probe is passed along the continuation of the artery, beside the sac, and is seen in the vessel again laid open as it lay along the wall of the phthisical cavity. The greater part of the tumour was free in the cavity, about one-third of it being supported with the vessel on which it was seated by the condensed wall of the cavity. The part of the sac connected to the vessel was its thickest portion, and from this the walls thinned away to the seat of rupture, a little to one side of the most prominent part of the tumour. The walls, though so thin, were stiff, and retained their form unaltered even after section. The position and size of the opening, as seen from within, are shown in the figure; but it is necessarily rendered much more distinct than it appeared when first seen.

It will be seen that in all its details this tumour agrees with the description published by Rasmussen, of aneurism of the pulmonary artery, as the disease occurs in connexion with phthisical cavities. The following abstract is taken from the translation of Rasmussen's paper published in the *Edinburgh Journal* by Dr. W. D. Moore:—"The size of these (aneurisms) varies considerably, from that of a walnut to that of a pea and under; they are formed by the dilatation of a vessel in contact with the inner wall of the cavity, the part of the vascular wall touching the cavity at the point of contact being dilated, while the remainder lies firmly embedded in the condensed vascular wall.

"The rupture takes place always at the most prominent point of the sac, and there is usually formed an irregular fissure-like rent, rarely exceeding in width two or three millimètres; most frequently it is only large enough to allow the knob of an ordinary probe to pass. In the opening adhere loose dark coagula, which are more rarely firm, and somewhat decolourized." "The vessels on which the aneurisms were located were, on an average, from one to three millimètres in width."

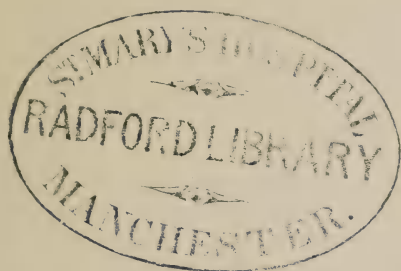
Since the publication of the above paper, several similar cases had been recorded in the medical journals, but this specimen was the first presented to this Society.—*April 1, 1871.*

Tubercular Disease of the Femur.—DR. BENNETT laid on the table an example of tubercular disease of bone, and a drawing which exhibited the appearances of the diseased bone while recent. The patient, a young man, from whom the specimen was taken, had died in Sir P. Dun's Hospital during the spring of last year. He had been admitted four

months before his death on account of extreme pain in the hip. He was not conscious of having received any injury in the hip, and attributed his pain to rheumatism. He was thin and pallid; although his appearance at once suggested the existence of phthisis, he did not complain of any chest affection, his sufferings from the hip appearing to absorb his whole attention. The examination of the chest revealed signs of advanced phthisis; and on being closely questioned, he admitted that he had had cough and expectoration for some time. A careful examination of the hip detected a very small collection of fluid over the external surface of the greater trochanter, in the position of the bursa which here covers that process. The hip-joint was freely movable, and not in any way implicated in the disease. The patient was unable to lie on the affected side, and any pressure on the trochanter was intolerable. As the case progressed, a marked change took place in the relations of the pulmonary and trochanteric diseases. The former became more and more distressing, and the attendant hectic more and more marked. The latter ceased to attract attention, and toward the close of the case the pain in the hip was not felt except on pressure, and then with none of its former acuteness. The effusion also diminished, so that it could hardly be recognized.

The patient sank at last, run down by uncontrollable diarrhoea, the leading symptom of his hectic fever.

The *post-mortem* examination proved the existence of most extensive tubercular disease of the lungs, which presented no special points of interest. The examination of the affected hip proved the disease to be strictly confined to the great trochanter and the structures immediately covering it. The portion of the bone normally covered by the bursa was bare, pale in colour, and at one point (*a* Plate 1) yielding on pressure; the borders of the diseased surface were surrounded by a fringe of thickened tissue, which in parts was excessively vascular, in others apparently infiltrated with yellow, tubercular matter. A vertical section through the bone, in a plane bisecting the great trochanter, exposed a circular patch of tubercular infiltration in the cancelli of that process. At first this appeared to be isolated from the superficial disease by a narrow strip of bone, free of deposit, as seen in Plate 2. A section made parallel to the first through the yielding spot (*a*) on the surface showed that at this point the two deposits of tubercular matter became continuous, and that the bone was here detached, and formed a true sequestrum. The cancelli of the trochanter involved in the tubercular deposit, in both parts, were whiter than those of the surrounding tissue; they were apparently deprived of vitality, though as yet they had not become detached, except at the one point (*a*). The specimen in all its details corresponded with the second form of tubercular disease of bone described by Nélaton—viz.: the tubercular infiltration, and with the





POSTERIOR VIEW.

ANTERIOR VIEW.

DR. R. W. SMITH—FRACTURES OF THE RADIUS AND ULNA.

variety of that form described as puriform or opaque. The coincidence of the disease with tubercular disease of the lungs was strong evidence of its nature, if such were required. The clinical history of the case is deserving of attention; for the more marked symptoms of the osseous disease existed at the time when we may conclude the deposit was in process of formation, and subsided as it became completed. This subsidence of symptoms may in part have been due to the progress of the graver disease of the lung and the general depression, but was no doubt in some degree attributable to the progress of the bone disease itself.—
April 15, 1871.

Double Fracture of the Radius and Ulna—one incomplete, the other complete.—Professor R. W. SMITH communicated the following case to the Society, and exhibited the specimen:—

A young man, aged eighteen, was caught by the strap of some machinery in an iron foundry, and whirled round with, of course, great violence and rapidity. He sustained the following injuries:—Compound fracture of the right humerus directly above the condyles; fracture of the right thigh; fracture of the tibia and fibula near the right ankle; compound fractures of the bones of the right forearm; enormous effusion of blood under the scalp, together with extensive contusions of the head, face, and right side of the thorax. He became comatose almost immediately after the occurrence of the accident, and died before two hours had elapsed. The head was not examined after death; but, judging from the immense effusion of blood under the scalp, and the presence of coma during the short time the patient survived, little doubt can be entertained of the existence of a fracture of the skull.

The specimen on the table consists of the bones of the right forearm, both of which are broken two inches above the radio-carpal articulation, and also at the height of five inches from the same joint. The forearm, prior to the removal of the bones, showed a remarkable deformity, which at once attracted observation. In the situation of the lower fractures, that is, two inches above the wrist-joint, the limb was bent at a considerable angle; the curve was abrupt, its concavity was upon the dorsal aspect of the limb, and presented a more striking appearance than the palmar convexity; the curve was fixed and permanent, and would have required the employment of considerable force to efface it.

Upon examining the bones carefully (after maceration), the lower fractures (which corresponded to the centre of the curve) were found to be *incomplete*. Upon the side of the convexity the solution of continuity was perfect until it reached the compact tissue upon the dorsal aspect of the curve, throughout the entire of which, as regards the radius, the bone was unbroken; but a transverse indentation marked the retiring angle

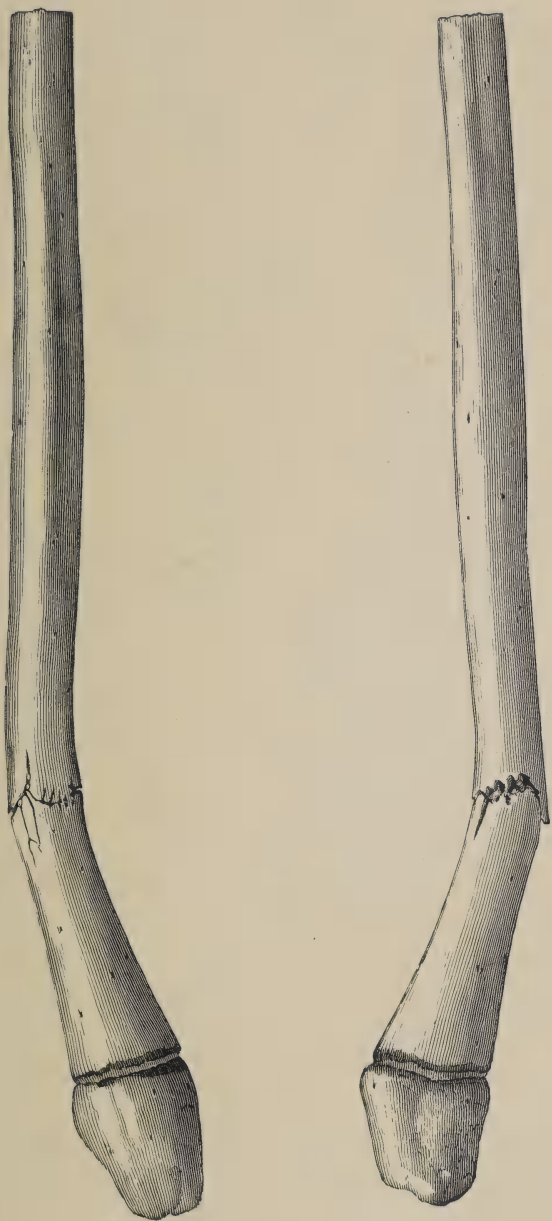
formed by the forcible bending in of the osseous fibres, the freedom of which, from actual fracture, was verified by the aid of a powerful magnifier. The extent of unbroken fibres was much greater in the radius than in the ulna. Along the external border of the radius a vertical fissure, two inches in length, extended upwards from the seat of fracture, while others, much shorter, passed downwards towards the wrist.

At the seat of the upper pair of fractures (five inches above the wrist), the limb was slightly deformed; but the curve was so slight as scarcely to attract notice. Here the fracture of each bone was complete, but that of the ulna was by far the more remarkable, and showed to perfection the mechanism of the injury, well named the "green stick or sally switch fracture." Where the bones first yielded, the fractures took a transverse course, till they reached nearly to the posterior surface. They then passed upwards, tearing strips of the compact tissue from the back of the upper fragments, that connected with the ulna being upwards of two inches and a half in length. The annexed plates represent accurately, and better than unaided description could give an idea of, the various osseous lesions noticed in this interesting specimen.

In December, 1851, I brought the subject of partial fractures of the long bones in young persons before the Society. The case which I then detailed I may, perhaps, be permitted to refer to briefly upon the present occasion, as the specimen is now illustrated by a very accurate plate. The preparation was presented to me by my friend Dr. Wade, who was acting as house-surgeon to the Derbyshire Infirmary when the accident occurred.

A boy, aged nine, sustained a compound fracture of the left leg, close to the ankle-joint, by a portion of a wall falling upon the limb. The nature and extent of the injury rendered immediate amputation necessary. The lower epiphysis was found to be detached from the tibia, and the bone immediately above it comminuted.

Dr. Wade, in his account of the dissection, stated that the fibula was *partially* broken two inches above its inferior extremity, and bent so as to present a convexity externally, and a concavity towards the tibia. This curve is retained even after maceration. Upon closely examining the specimen, however, I found that, although the bone still retained its curved form, the fracture was *complete*, and as manifest upon the side of the concavity as upon that of the convexity. In the latter direction there was fracture with separation of the fragments; in the former, fracture with forcible approximation of the extremities of the fragments, the osseous fibres of which, in this situation, seemed to be dovetailed into each other, forming a suture, of a most delicate character, it is true, but firm enough to enable the bone to maintain the curved form imparted to it by the injury. (See Plate.)



DR. R. W. SMITH—FRACTURE OF THE FIBULA.

From the two cases just described, it is evident that the phenomena which accompany incomplete fractures, such as curvature, the persistence of the curve for a longer or shorter time, the cracking noise usually heard when the bone is straightened, &c., are also present in cases where the fracture is complete, but where the fragments are firmly held together by their assuming a denticulated or suture-like arrangement at the concavity of the curve; the difficulty thus caused of forming a differential diagnosis is not, however, a matter of importance, the same principles of treatment being applicable to each form of injury.—*April 22, 1871.*

Lithotomy in the Female.—The Vesico-Vaginal Operation. Two papers translated from the *Hospitals-Tidende*. By J. W. MOORE, M.D., M.Ch., Dub.; L.K.Q.C.P.I.; Ex-Schol., T.C.D; Honorary Fellow of the Swedish Society of Physicians.

I.—*Case of Vesical Calculus in a Woman—Removal by Vesico-Vaginal Incision—Bringing together of the Wound by Sutures—Healing by First Intention.* (Dr. Plum, *Hospitals-Tidende*, June 7th, 1871.)

As is well known, vesical calculus is far less frequently met with in women than in men. Anatomical conditions very naturally explain this, since the short and wide urethra of the female permits small stones to pass much more easily than is the case in men. Nor is it so very small a stone which can thus be got rid of in a natural way, for in the literature of the subject cases are found reported where stones several ounces in weight, several inches in circumference, have been passed spontaneously through the female urethra.

If it is thus but seldom that in practice we meet with a vesical calculus in a female—yet such cases do occur from time to time. But even then a woman is more fortunately circumstanced than a man, since the operative proceedings which are necessary for the purpose of removing the stone in the female are less intrusive and dangerous than in the male.

Besides the two principal methods of operation, which are at our command in cases of calculus in the male—lithotomy and lithotrity—in the female we further possess a third expedient which has been resorted to in a very considerable number of instances, namely, dilatation of the urethra. This operation, which very naturally resulted from the experience that the female urethra may often be dilated to a very considerable extent, has been carried out in different ways, at times as slow gradual dilatation, by means of sponge-tent and sea-tangle; at times as

rapid dilatation, by means of suitable instruments constructed for the purpose, such as Cooper's two-bladed and Weiss's three-bladed dilators. In addition to these two methods certain operators have even proposed to use the calculus itself as a dilator, having first grasped it with a forceps introduced into the bladder, a method which is indeed applicable in the case of a smaller stone, but which appears to me rather rude and inadmissible where the stone has a larger circumference.

It is quite true that a stone, itself of no small magnitude, can be removed by the method of dilatation, and within certain limits that method may even be regarded as free from danger; but the disadvantage attaches to it that in several cases, especially after prolonged dilatation, a permanent incontinence of urine has remained as a consequence. As a matter of course, in the case of a very large stone, the question of employing dilatation cannot be entertained, whereas with smaller stones, which are too hard to crush, the operation may undoubtedly be indicated.

The method of operation, which may be considered the principal one in the treatment of vesical calculus in women is certainly that of *lithotripsy*. This procedure is far more easily carried out in their case than in that of men. The short, straight, and wide urethra enables the operator to manœuvre easily with the lithoclast in the bladder; there is no prostate; and the fundus of the bladder is dilated downwards somewhat pouch-fashion, so that an assistant, by introducing his finger into the vagina, can aid in lifting up the stone between the blades of the instrument. Lastly, the crushing does not require to be so fine in the case of women as in men, as even the larger fragments can readily pass out through the wide urethra. The lithoclast is also admissible in the case of female children. Fergusson, for example, has repeatedly employed it with success: amongst other instances, in a little girl of five years, in whose case in two sittings, at an interval of a fortnight, he crushed and removed a stone. The only inconvenience that this operation labours under in women more than in men is that the patient frequently experiences difficulty in keeping back the urine during the process, so that it strains out at the side of the lithoclast, and that without the possibility of preventing it, as in men, by compressing the urethra against the instrument.

It is certainly quite beyond dispute that we would prefer lithotripsy in every case of vesical calculus in a woman if this operation were admissible in all instances, but this unfortunately is not so. Sometimes the stone is so large, or so hard, that it is impossible to crush it, and in other cases a condition of the bladder—a considerable inflammation or irritability—affords a contra-indication for lithotripsy. In such cases, if we except dilatation of the urethra, we have only the operation of cutting to fall back upon, and this too is very often practised, numerous methods of various kinds having been described in the case of women as in that of men.

The high incision over the symphysis pubis has been resorted to. The anterior wall of the bladder has been opened by a transverse incision between the urethra and symphysis, the so-called “vestibular” operation of *Lisfranc*, a procedure which is so very objectionable in all respects that it is now scarcely ever used. Urethro-vesical incisions in all possible directions have been made—upwards, downwards, outwards—most frequently, indeed, obliquely outwards and downwards towards one or both sides, corresponding to the lateral and bi-lateral incision in man. Lastly, the bladder has been entered from the vagina by the vesico-vaginal incision, or “kolpo-cystotomy” as it is called.

If anatomical conditions in women are considered, and if we seek for a place where the bladder may be opened to a sufficient extent without wounding the peritoneum, or any large vessels, the last-mentioned operation, that by a vesico-vaginal incision, appears to stand forth as the easiest and most natural. And if it has not hitherto been very extensively employed, the reason certainly is this, that we were formerly helpless against vesico-vaginal fistula, which was regarded as, and really was, an almost unavoidable consequence of the operation. We, in performing it, secured a new complaint for our patient, a complaint in no wise inferior to that from which she had been set free. This state of things has, however, become quite altered with the progress of our time in this direction.

The successful results which have been obtained of late years from the operation of vesico-vaginal fistula, according to the American method, should naturally place kolpo-cystotomy under far more favourable circumstances than hitherto, for we can now be pretty nearly certain of being able to close the fistula produced by the operation, and if the apprehension of causing a permanent vesico-vaginal fistula is once removed, it seems to me that all objections to kolpo-cystotomy must fall to the ground.

The length of the anterior wall of the vagina is sufficient to enable us to form an opening large enough to remove a stone of fully as great a size, at all events if we divide it in pieces, and that without coming into collision with important anatomical parts, without any special hæmorrhage when the incision is made in the middle line.

This operation has now also been adopted anew in most countries, and if, in the literature of late years, we meet with only individual communications respecting it, this no doubt arises alone from the rare occurrence of such cases—just as the reason why some very recent surgical authors, as for example Linhart and Emmert, seem not to share the view here propounded of the subject, and in their text-books devote but a few words to kolpo-cystotomy, is no doubt alone to be sought for in the very small experience of these authors in this procedure.

After having learned from the operation for vesico-vaginal fistula how

readily even considerable deficiencies in the vesico-vaginal wall may be closed by a careful paring and accurate adaptation of the edges, we may, without losing sight of the subject of kolpo-cystotomy, go even a step further, and endeavour, by all means, to prevent the formation of fistula, by bringing together the wound with sutures, immediately after the removal of the stone. The operation has been successfully practised in this way, first by Marion Sims, and by Vallet, of Orleans; in England for the first time, with a partially successful result, by James Lane, in 1862, after Paget, of Leicester, had previously tried it, without the union being completely successful. Subsequently several satisfactory operations of this kind have been performed by Lyon, of Glasgow, Aveling, of Sheffield, and Baker Brown.

The only condition under which the operation appears to me necessarily to be exposed to considerable difficulties is in children; but even in this case experience shows that the difficulties are not insurmountable. Fergusson has removed a mulberry calculus the size of a walnut from a girl $9\frac{1}{2}$ years of age by kolpo-cystotomy, while the attempt to close the wound with sutures failed. Paget has undertaken the operation in a child 3 years old, and Simon, lastly, has communicated in the 12th volume of Langenbeck's *Archiv für Klinische Chirurgie* a very interesting case, in which he succeeded in a girl 8 years of age—certainly with much difficulty and only after repeated operations—in closing a vesico-vaginal fistula $5\frac{1}{2}$ centimetres in length, that had been produced by the ulceration of a calculus through the wall of the bladder.

Kolpo-cystotomy has indeed been performed on several occasions in this country (Denmark). I have at least once assisted at an operation for a vesico-vaginal fistula attributable to this operation, but so far as I am aware, up to the present no operation of this kind has been undertaken here with immediately succeeding closure of the incision wound. As in the summer of last year I performed this operation with perfectly favourable results, I will venture to communicate the case in question, in the belief that in many respects it presents features of interest.

Mrs. M., aged 35, was last confined some eight years ago. During her confinement a violent attack of parametritis, with formation of pelvic abscess, became developed. This abscess, after a very long lapse of time, led to the formation of a fistula in the right groin, communicating with the intestine, which has since been present. The patient had for a long time been obliged to keep her bed, and as her strength was considerably reduced, she was very freely supplied with nourishment, and again began to recover. In the course of the last five years she has, apart from the affection described, shown symptoms of a vesical affection, with pains in the region of the bladder, frequent and painful micturition; but these symptoms were attributed to the other already existing malady, until Dr.

Howitz, who had at the time undertaken the treatment of the patient, in the spring of 1870 detected a stone in the bladder, and requested me to perform the necessary operation for this affection.

When I saw the patient for the first time in the month of May she was tolerably emaciated, pale, and weak, complaining of great pains in the bottom of the pelvis, with very frequent and painful calls to pass water. The urine indicated the presence of a vesical catarrh, and on examination with instruments a large stone, which could also be felt perceptibly through the vagina, was found in the bladder.

Although the stone was large I yet believed myself called on to attempt its removal by lithotrixy, which operation was accordingly performed on several occasions, at intervals of two or three days. I succeeded repeatedly in obtaining a firm grasp of the stone, and in breaking it, partly by percussion, and partly by drilling. But the calculus was very hard; it was only very small fragments that it was possible to remove in this way, and, what was the worst feature in the case, each introduction of the lithoclast, and the manœuvring with it in the bladder, were accompanied with excessive pain, so that the separate sittings were necessarily but very brief, the patient almost the entire time screaming aloud with pain at each movement of the instrument.

It was only after many persevering attempts to effect my purpose by this means that I was obliged to give up lithotrixy. I then determined to make a vesico-vaginal incision, and to attempt an union of the wound immediately after the stone's removal. This operation was performed on July 5th, 1870, in the following manner:—

The patient, having been chloroformed, was placed on her side, lying slightly over on her abdomen, with thighs well drawn up, and hips advanced. A Sim's speculum was introduced into the vagina, and an assistant held it well back, in apposition with the posterior wall of the passage. By this manœuvre the site of the operation became plainly visible, and readily accessible. As a guide in making the incision, for which a straight director is generally used, or an ordinary large-sized sound, I employed the old apparatus used by Guérin in the lateral incision. I will remind those of my readers who may possibly have forgotten the construction of this apparatus, that it consists of an ordinary curved staff, from the foremost extremity of which projects a blade, which runs down parallel with the staff, and terminates at a level with the place where the nozzle of the staff begins. Through the end of this blade a stilette, grooved on its under border, may be advanced to meet the groove at the curve of the staff, in which position it is then fixed by means of a screw. The design of this instrument is to guide the lithotome with certainty into the groove of the staff.

When a staff of this description, whose nozzle was only a full inch long, had been introduced into the bladder, the stilette was advanced, and the

vesico-vaginal wall was perforated in the middle line an inch behind the external orifice of the urethra, and when its point had reached the groove of the staff, the stilette was fixed in this position. The apparatus was now grasped with the left hand, and while the vesico-vaginal septum was stretched firmly downwards, a strong knife was carried forward in the groove of the stilette, and from it to the groove of the staff, the vesico-vaginal septum being thus severed by an incision of a full inch in length in the middle line.

After removing the apparatus, I dilated the incision slightly upwards with a strong pair of scissors, and the stone could now easily be extracted with the aid of the little finger alone.

The calculus had been divided (by the crushing) into two parts, of about equal magnitude. It was in its entirety six centimetres in length, five centimetres in breadth, and two centimetres in thickness, and, together with the small portions which had previously been removed in the crushing, weighed 42 grammes (648·1 grains). On section it presented a regular series of alternate brown and white strata, as is generally the case with a calculus consisting of layers of uric acid, urates, and phosphates.

There was only slight bleeding, which was easily staunched by means of cold injections; and when the patient had perfectly wakened from the chloroform stupor she was put to bed, where she remained quietly for an hour. She was then again laid upon the operating table in the position already described. Sim's speculum was introduced, and I now brought the wound accurately together with silver wire sutures, which embraced its tolerably broad edges, without, however, penetrating through the vesical mucous membrane. Ten sutures in all were introduced, being closed in the usual way with leaden plates, which were compressed round the taught wire. The vagina and bladder having been washed out with lukewarm water, the patient was put to bed.

During the following days the urine was drawn off by a catheter every second or third hour, and twice a-day the bladder was washed out with a lukewarm, very dilute solution of carbolic acid. No untoward accidents of any kind occurred. The catheterization was at first rather painful, and in the earlier period of treatment some mucus came away from the bladder, but this symptom also soon disappeared.

On the 12th July, the seventh day after the operation, I removed five sutures, and the wound was then perfectly healed, and appeared only as a whitish streak. On the twelfth day the remaining sutures were taken away, and the patient got up, shortly afterwards returning to the country. There she soon regained health and strength, was free from the greatest part of her former sufferings, the little intestinal fistula, of course, remaining unaltered.

This clinical history, which I have here briefly reported, appears to me

—as I have already said—to present features of no small interest, as fulfilling every reasonable claim to an operation of this kind. When I performed the operation with immediately succeeding union of the wound, I was principally incited to do so by my successful experience of operations for vesico-vaginal fistulæ, without my having as yet had a more thorough acquaintance with the results of the operative attempts in this direction above mentioned, and I will not deny that I looked forward to the result with a certain degree of anxiety. I could not keep from alarming myself with a conception of a possible infiltration of urine into the connective tissue between the mucous membranes of the bladder and vagina, especially in an upward direction. For while one might *à priori* imagine himself well-off with an accurate union of the wound in the mucous membrane of the vagina, the wound in that of the bladder might not have become so accurately united. Fortunately this anxiety proved to be superfluous, for the wound healed perfectly by first intention, and a further proof of this is, that the urine—even, as in this case, from a catarrhal bladder—exercised no injurious influence on the wound with respect to delaying its healing, although it had for more than an hour after the operation moistened the edges of the wound.

I shall, further, merely venture to direct attention to the use I made in the present instance of Guérin's apparatus. By this the performance of the operation is rendered extremely easy and simple, chiefly by enabling the operator to take a firm hold of the vesico-vaginal septum, and by permitting that structure to be made conveniently tense in the direction of the incision. For, where we cut in freely on a sound, or an ordinary staff, we may certainly have some difficulty in making the incision, and in giving it the determined direction and extent. For some time I flattered myself that the employment of this apparatus in this way was my own discovery, but I have since found, in more closely investigating the question, that so far back as 1812, Michaelis used Guérin's apparatus in a similar manner in the operation of kolpo-cystotomy.

II.—*Case of Vesical Calculus in the Female—Operation by Vesico-Vaginal Incision—Re-Union by First Intention.* (Hr. Selchau, Practising Physician at Grenaa, *Hospitals-Tidende*, January 24, 1872.)

THE patient is 32 years of age, and has been confined four times, on the last occasion, in September, 1871. From May of that year she has complained of severe pains during, and particularly after micturition; bearing down sensations; discomfort while sitting, and pain at the lowest part of the abdomen on change of position. She had, moreover, even from the time when she was about 16, noticed some smarting and pain on making water—symptoms which became decidedly more marked whenever she caught cold, and at the menstrual period. During the last

portion of her pregnancy the pains on micturition have been more violent and more constant, and she has suffered extremely, while her strength has greatly given way, and she has been much distressed from want of sleep. After her confinement, which took place early in September, and was quite natural, the relief in the symptoms expected by her did not appear, and I was accordingly called to see her on the 29th of last November, when I placed her under treatment.

At this time she presented all the signs of a violent irritation of the mucous membrane of the bladder, and on introducing a catheter, I was easily able to establish the presence of a calculus. It lay just to the right of the entrance to the bladder, and as at the same time it could readily be felt through the vagina, I was at once able to determine that it was of no inconsiderable size. I directed the patient to be brought to the town where I reside—she lived about 6 English miles from it—and I then made repeated attempts, with a view of crushing the stone, but although anatomical conditions do indeed in a high degree facilitate lithotrity in the female, yet I was not successful in seizing the stone in an axis favourable enough to allow of the question of any energetic attempt at crushing being entertained. On the contrary, each effort—although I am conscious of having used the greatest gentleness—evoked a very severe irritation of the mucous membrane, which lasted for some time, and appeared to me an indication against any further attempt, especially as I assumed the stone to be of a tolerably considerable size.

I determined, therefore, to remove the calculus by a vesico-vaginal incision. I performed this operation on December 17th, with the kind assistance of Dr. Krohn, of Boende, and of Dr. Richter, of this town. After the patient had been chloroformed, lying on her back, she was turned on her left side. I then introduced Sim's speculum, which exposed very well the site of the operation. As a director I availed myself of the female blade of the lithoclast, by means of which the partition-wall between the vagina and bladder was brought to project quite sufficiently, and at the fenestrated portion of the instrument I made in the middle line a straight incision which was then easily a little dilated, merely by a slight advance of the director, so that the entire incision was an inch and a-half in length. Through this opening a somewhat flattened calculus was taken out with the fingers' aid. The bleeding, which was not abundant, was staunched with two or three syringings of cold water, and the patient was allowed to rest for three-quarters of an hour, during which time she completely awakened from a tolerably deep stupor, and was otherwise a little looked after. After the lapse of the time mentioned she was again turned on her left side, when the wound was brought together without the use of chloroform, by means of eleven sutures of silver wire, which were closed by twisting. The whole operation, including the period of rest, extended over two hours.

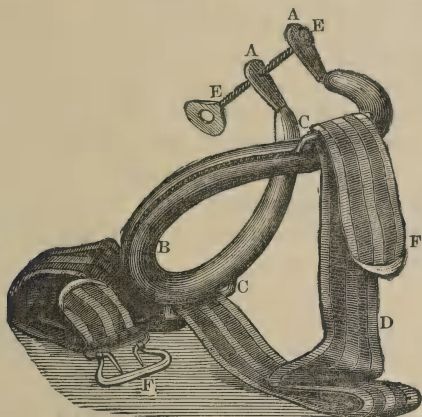
The after treatment consisted in the thorough washing out of the bladder and vagina twice a day with tepid carbolic water (one part of carbolic acid to 100 of water), and in cleaning of the catheter, which was left inserted. This was, however, submitted to for two days only, after which it was necessary to substitute catheterization every third hour. On the 9th day after the operation three alternate sutures were taken away; on the 11th day I removed another, which supplicated a little; on the 14th day the remaining seven were removed, two of which on the 11th day had shown commencing suppuration, but of this I found no further trace at the time of removal, having on the intervening days used a tampon of carbolic glycerine.

After the removal of the last sutures, the patient remained a week longer under my observation, during which time I had an opportunity of determining that even with strong forcing down with a full bladder there was not the slightest permeability to be detected. Meanwhile, as a matter of course, the patient was free from her former inconveniences, with the exception, naturally, of a great debility and tendency to sleep. Both these symptoms I ascribe, however, to her anæmic and enfeebled condition.

The calculus was triangular and rather flattened, measuring in its longest diameter one and a-half inches, on the other two sides being one and a-third, and one inch in length, respectively. It weighed 16·5 grammes (254·63 grains), and consisted of a round nucleus, which, through oblique slanting off of the exterior surface, had produced the triangular form. I placed it in the lithoclast, and with even pressure easily broke up the exterior shell into fragments, but the nucleus withstood both the even pressure exercised by the drill, and violent percussion with a hammer. After this result of the crushing, I could but congratulate myself at not having been more persisting in the attempts to crush the stone. For by this procedure I should have been able to rid the patient only of the shell, while the nucleus would, notwithstanding, have necessitated the further operation. And to this operation I had quite made up my mind in the present case, in consequence of the patient's extraordinary delicacy, both during and after each sitting for crushing. This delicacy also seemed to me, necessarily, to add its weight as an indication for an operation, which—having regard to the history of vesico-vaginal fistula of the present time—may well be termed dangerous. The risk, *à priori* most probably thought of in connexion with the ordinary union of the edges of the wound in which very possibly the vesical mucous membrane may not participate, is that brought forward by Dr. Plum in his case detailed in the *Hospitals-Tidende* for June 7th, 1871, namely, infiltration of urine. But this risk is considerably lessened by the insertion of a fixed catheter, through which—as the well-known English author, Sir Henry Thompson, so happily expresses himself—the ureters are prolonged without the body.

In my case, no doubt, the patient did not tolerate the permanently inserted catheter for more than three days, and although I would not venture to assume that within that time any solid union between the wounded edges of the mucous membrane had occurred, at all events so close an agglutination had taken place that the urine did not act injuriously on these wounded edges—or we may even pronounce the urine contained in a catarrhal bladder to be destitute of any such hurtful influence as that to which for so long a time it has had a prescriptive right.

NEW PREPARATIONS AND SCIENTIFIC INVENTIONS,
IN RELATION TO
MEDICINE, SURGERY, AND HYGIENE.

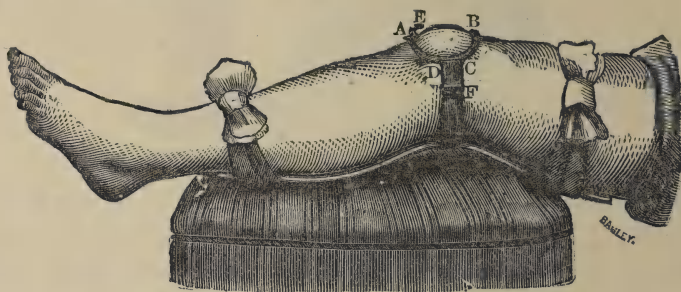


Dr P. S. O'Rielly's Patella Splint.

A NEW APPARATUS FOR FRACTURES AND DISLOCATIONS OF THE PATELLA.

WE have much pleasure in calling attention to an apparatus for the treatment of fractures and dislocations of the patella, which has been invented by Dr. P. S. O'Rielly, of Saint Louis, Mo., who has favoured us with the following description of it, and with the electrotypes illustrative of it. He says:—"This splint consists of a piece of rounded, tempered steel, and, as may be observed from the illustration, is of an oval or horse-shoe shape, with the ends turned in and bent upwards. The sides are depressed so that the part corresponding with the toe (B) of the shoe is curved upwards, to the extent of about half an inch, allowing it (the toe) to ride over and upon the ligaments of the rectus muscle, while the sides dip downwards and embrace the bone, and at the same time, the splint, by pressure at the insertion of the vasti muscles, counteracts the action of the latter; and nevertheless, does not by undue pressure upon the tendon of the rectus muscle, cause the fragment to tilt up at the point of fracture, as necessarily results when the pressure is made on this tendon alone, as in the use of the simple ring. On the sides, slightly anterior to a line through the centre of the oval, or horse-shoe part, are fixed ears or loops (C) for the reception of the band (D) by which the shoe portion of the splint is held in position. Through the turned up end of that portion corresponding

with the heel or calk (A) of the horse-shoe, is a thumb-screw (E) by which the splint is compressed or expanded as may be required. The splint being covered with chamois or soft felt and a strap of silk or linen webbing (D), or smooth leather, with a buckle (F) on one end, the free end of the strap is made to pass through one of the ears, rendering it complete for application. The injured limb being laid upon one of Day's curved posterior splints, well padded, and a little wider than the leg, secured at each end by a handkerchief or roller bandage, the fragments then being brought into position the *Patella Splint* is placed over and made to encircle them: the strap (D) is carried under the leg and around Day's splint and run through the opposite loop or ear, and returned upon itself and buckled (F) sufficiently tight to keep the splint in position, but not so much so as to inconvenience the patient more than is necessary. By the thumb-screw (E) the splint may be tightened or relaxed as circumstances demand. Cold applications can now be applied *ad libitum*.



Dr. P. S. O'Rielly's Patella Splint applied.

"In the case of dislocation of the patella upwards, in the rupture of the ligamentum patellæ, from the tuberosity of the tibia, by means of the attachment of an elastic strap to the ears or loops, and by passing it under the sole of the foot, the most perfect approximation may be secured. The ears or loops, which are manufactured of hard, tempered steel, should extend out at least a quarter ($\frac{1}{4}$) of an inch from the body of the splint, so as to keep the strap out from the leg, obviating the pressure and thus preventing constriction upon the cutaneous vessels.

"I would state, without intending to offend the intelligence of any of the critics that the "*splint*" is not a ring, at least according to the ordinary conception of the meaning of the term, or in accordance with the definition of the leading lexicographers of the day. As the patella bone is not round, but of a triangular or heart-shaped figure, a splint that embraces its fragments ought to be as near its shape as possible in order that it may be successful. This fact may account for the failure of the Purmann's ring and have caused it to fall into partial disuse."

PERIODICALS WITH WHICH THE DUBLIN JOURNAL IS EXCHANGED.

GREAT BRITAIN.

1. The British and Foreign Medico-Chirurgical Review. Churchill.
2. The Edinburgh Medical Journal. Oliver and Boyd.
3. The Retrospect of Medicine. Edited by W. Braithwaite. Simpkin, Marshall, and Co.
4. The Half-yearly Abstract of the Medical Sciences. Churchill.
5. Pharmaceutical Journal. Churchill.
6. The Lancet.
7. The Medical Times and Gazette. Churchill.
8. The British Medical Journal.
9. The Asylum Journal of Mental Science. Churchill.
10. The Glasgow Medical Journal. Dunn and Wright.
11. The Athenæum.
12. The Dublin Medical Press.
13. The Westminster Review. Trübner.
14. Transactions of Obstetrical Society. London: Longmans.
15. Journal of Cutaneous Medicine and Diseases of the Skin. Edited by H. S. Purdon. Churchill.
16. The Practitioner; a Monthly Journal of Therapeutics. Macmillan and Co.
17. The Journal of Anatomy and Physiology. Macmillan.
18. The Food Journal. London: J. M. Johnson and Sons.

INDIA.

19. The Madras Monthly Journal of Medical Science. Madras: Gantz, Brothers. London: Hardwicke.
20. Indian Medical Gazette. Calcutta: G. Wyman and Co.

AUSTRALIA.

21. The Australian Medical Journal, Melbourne: Stillwell and Knight. London: H. Baillière.

AMERICA.

22. The American Journal of the Medical Sciences. Edited by Isaac Hays, M.D. Philadelphia: Henry C. Lea. London: Trübner and Co.
23. The American Journal of Science and Arts. Conducted by Professors B. Silliman, and J. D. Dana, &c. New Haven: Editors.
24. The American Journal of Insanity, Utica, N. Y. State Lunatic Asylum.

AMERICA.—Continued.

25. The American Journal of Obstetrics and Diseases of Women and Children, New York: W. A. Townsend and Adams. London: Trübner and Co.
26. The Cincinnati Lancet and Observer. Cincinnati: E. B. Stephens, M.D.
27. Canada Medical Journal. Montreal: Dawson, Brothers.
28. The New York Medical Journal. New York and London: D. Appleton and Co.
29. The Medical and Surgical Reporter. Philadelphia: S. W. Butler, M.D.
30. The Richmond and Louisville Medical Journal. Louisville, Ky.: E. S. Gaillard, M.D.
31. The Medical Record. New York: Wood & Co.
32. The New Orleans Journal of Medicine. New Orleans: W. S. Mitchell, M.D. London: Trübner and Co.
33. The American Practitioner. Louisville, Ky.: John P. Morton and Co. London: C. D. Cazenove.

FRANCE.

34. Journal de Chimie Médicale, de Pharmacie, de Toxicologie, et Revue de nouvelles scientifiques, nationales et étrangères, &c. Paris: Labé.
35. Journal de Médecine de L'Ouest. Nantes: Mellinet.
36. Journal de Pharmacie et de Chimie, &c. Paris: Victor Masson.
37. Revue Médicale Française et étrangère. Publié par le Docteur Sales-Girons, Paris.
38. Archives Générales de Médecine. Paris: Asselin.
39. Bulletin de l'Académie Impériale de Médecine. Paris: Baillière.
40. Revue de Thérapeutique Médico-Chirurgicale. Paris: Dr. A. Martin-Lauzer.
41. Journal de Médecine et de Chirurgie Pratiques a l'Usage des Médecins. Par Lucas-Championnière. Paris.
42. Journal des Connaissances Médicales Pratiques. Paris: J. B. Baillière et Fils.
43. Annales Médico-Psychologiques. Par MM. Baillarger, Cerise, et Lunier. Paris: V. Masson.
44. Bulletin Général de Thérapeutique, Médicale et Chirurgicale. Par le Docteur Félix Bricheteau. Paris.

FRANCE.—*Continued.*

45. Répertoire de Pharmacie. Par M le Dr. Bouchardat. Paris: G. Baillière.
 46. Gazette Médicale de Strasbourg.
 47. Journal de Médecine de Bordeaux.
 48. L'Union Médicale de la Gironde, Bordeaux.
 49. Lyon Médical Organe Officiel de la Société Impériale de Médecine. Lyon: Mégret.
 50. Journal de Médecine Mentale. Par M. Delasiauve. Paris: Masson et Fils.
 51. Archives de Médecine Navale. Paris: J. B. Baillière et Fils.
 52. Archives de Physiologie Normale et Pathologique. Publiées par MM. Brown-Séguard, Charcot, Vulpian. Paris: Victor Masson, et Fils.

BELGIUM.

53. Bulletin de l'Académie Royale de Médecine de Belgique, Bruxelles.
 54. Annales D'Oculistique. Bruxells.
 55. Annales et Bulletin de la Société de Médecine de Gand.

GERMANY.

56. Vierteljahrschrift für die praktische Heilkunde, herausgegeben von der medicinischen Facultät in Prag. Prague: Karn André.
 57. Canstatt's Jahresbericht über die Fortschritte der gesammten Medicin in allen Ländern. Redigirt Von Pr. Scherer, Pr. Virchow, und Dr. Eisenmann. Würzburg: Stahel.
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PART I.

ORIGINAL COMMUNICATIONS.

ART. XIII.—*Case of Measles Associated with Hæmorrhagic Variola.*

By CHARLES H. ROBINSON, M.D.; Demonstrator of Anatomy to the Ledwich School of Medicine; Member of the Surgical Society of Ireland, Pathological Society of Dublin, &c.

On the evening of the 5th March last I was sent for to see H. T., a young man aged twenty-three, who was said to be very feverish, and suffering from uncontrollable vomiting. It seems that he had felt unwell the previous day, and was so bad on the morning of the 5th that he had not gone to his business, but remained in bed until I saw him. He was then suffering excessively from nausea and vomiting, nothing remaining on his stomach; the pulse was 87, strong and bounding; temperature in axilla, 99°; skin hot and dry; patient tormented with thirst; eyes injected, face flushed; lachrymation; *no pain in the back*, nor indeed anywhere except in forehead, and at the roots of the hair; bowels not opened for several days. He was ordered ten grains of compound colocynth pill, to be taken at once, a cantharides blister to epigastric region, and five grains of the trisnitrate of bismuth every third hour; whilst to alleviate the thirst and remove the vomiting, small pieces of ice were directed to be constantly kept in his mouth, and allowed slowly to dissolve there.

6th March (3rd day of illness), 10 a.m.—Pulse 93; temperature $99\frac{3}{4}^{\circ}$; stomach still irritable; is more feverish and excited this morning, partly in consequence of having last night drank some port wine and whiskey (two glasses of the latter I understand), which, being a teetotaller, had a great effect upon him. No eruption anywhere, no pain except in head; aspect not so clear; had delirium of a mild kind last night, and did not sleep at all; continue ice, and to take small quantities of beef-tea every ten minutes. He vomited the colocynth pills immediately after taking them, so placed a dose of calomel (gr. v.) with sugar on his tongue. Skin very hot; urine high coloured and scanty; ordered a diaphoretic mixture, containing the solution of the acetate of ammonia, and some nitric spirits of ether, to be taken every third hour. When the stomach is empty the vomiting is simply mucus, with a little bile. 6 p.m.—Pulse 84; temperature $99\frac{3}{4}^{\circ}$; was only sick twice since this morning; the stomach is able to retain almost all the beef-tea taken; the blister, although left on for twelve hours, did not rise, and only reddened the skin; bowels moved freely since morning; no cough.

7th March (4th day of illness).—Pulse 90, weak; temperature $102\frac{3}{4}^{\circ}$; face flushed, of a dusky crimson colour, also hands, whilst an eruption of measles is to be seen on arms and lower extremities, the latter being thickly covered with it. This eruption is crescentic and apparent to the touch, presenting the characteristic appearance of irregular semicircles, with clear skin in the centre. At the same time with this eruption there was an exanthematous rash well marked over various parts of the body, the head and neck excepted, the abdomen, especially where mustard poultices had been applied, the chest slightly, resembling scarlatina. I may also observe that the nail drawn rapidly over this rash did not remove the redness longer than for a couple of seconds. The axillary or inguinal glands were not enlarged. No itching whatsoever, no sore throat, nor any chest symptoms; eyes bloodshot, nausea still, but no vomiting since last night; did not sleep last night, but no delirium existed. 6 p.m.—Pulse 96; temperature $103\frac{3}{4}^{\circ}$; copious eruption; no pain anywhere; slept a little since morning. To take a dose of Dover's powder at bed-time. I may mention that when I saw him this morning I was asked, should he feel better, might he leave bed and lie down on a sofa below stairs for a short time. I distinctly told the person in charge that on no account was he to leave his bed; but to my astonishment on paying the evening visit

I found him in a room below his bed-room, dressed, lying on a sofa, and cramming himself with all sorts of food, cakes, &c. As I helped him into bed, he complained of a sense of weight in the back rather than pain.

8th March (5th day of illness).—Pulse 102; temperature $103\frac{1}{2}^{\circ}$; much worse; violent delirium last night; hæmorrhage from kidneys to a large amount, also epistaxis; eyes greatly ecchymosed, and a good deal of mucus collecting in throat. I saw him this morning in consultation with Mr. Wharton, who gave a very unfavourable prognosis, agreed with me as to the measles, but thought also that there might be some latent small-pox present. As the lodging-house where he resided did not give him sufficient attendance, and also as he possessed no relatives in Dublin, it was thought advisable to have him removed to the pay ward of Sir Patrick Dun's Hospital, where he was admitted the same day under Dr. Aquilla Smith, to whose courtesy I am indebted for any further information, and who obtained for me the following facts from the notes of his clinical clerk, Mr. Jones, which that gentleman kindly furnished me with.

March 8th (evening).—The face was somewhat swelled and of dark red (brick red) colour; intense ecchymosis of both eyes. The whole of the upper extremity presented the same appearance as the face, his lower extremities were thickly covered with petechiæ of various sizes. The eruption was not elevated* on any part, with the exception of some characteristic papulæ on the lower extremities, with this exception, the eruption was of a wavy (rough) appearance. He had both hæmatemesis and hæmoptysis, and these seemed to be his most distressing symptoms. In addition to these he had also melenæ and hæmaturia, slight headache, and a sort of weakness in the back, as he described it. Pulse 102, full, quick, and regular; temperature $104-6^{\circ}$.

9th March (6th day of illness).—Pulse 122, not full but regular; temperature $101-2^{\circ}$; in the evening the pulse was 124, scarcely perceptible, and not very regular; temperature $102-1^{\circ}$. He was perfectly collected until his death, which took place at 9 o'clock on the morning of the 10th March, but seemed to answer questions hurriedly when asked; this was partly due to his incessant vomiting of blood and the contents of his stomach; his breathing was also a little hurried. On passing the hand on his

* If the eruption was not elevated how could it feel rough?—C. H. R.

chest it left a white impression, which lasted about six seconds after the hand had been removed. The characteristic smell of small-pox was also present.

Such is the clinical history of this case, and I have thought it advisable to be thus minute in detailing every symptom and occurrence in this very interesting case, in order that proper deductions should be drawn as regards the diagnosis. The occurrence of measles with variola is very uncommon, but, according to Cazenave, may co-exist in the same individual, the progress of one of the eruptions, under such circumstances, being generally arrested by that of the other.

I should observe that this patient stated that he had an attack of measles before, and also variola (as well as I can remember he was marked slightly on the forehead from the latter), but had never been vaccinated. That he had the measles before is not of much importance, for although rare, yet an individual may be affected twice with this eruption.

I may confess that but for a couple of symptoms to which I shall advert hereafter, I should have called this case one of malignant measles; and in order to show that the symptoms above described were in accordance with those mentioned by authors on this disease, I shall give a few extracts from medical writers of repute as justifying this view.

Dr. Churchill, of this city, in his well-known work on *Diseases of Children*, p. 640, states that in the typhoid or malignant form of measles the pulse is quick, the skin dry and burning, and that petechiæ may be observed on different parts of the body. Dr. Gregory thus alludes to this variety:—"The stomach is irritable, vomiting is both severe and protracted; there is delirium with wildness of eye. . . . On the surface appear petechiæ or ecchymosed patches of eruption; blood passes by stool, &c." Hebra^a also describes this form of measles as follows:—"There is some depression from the very commencement of the disease. The fever is continuous, and increases day by day. The pulse is at first hard, but soon becomes compressible, and the artery has then an empty feel. The skin is sometimes hot and dry, or covered with perspiration, and there is intense thirst. The patient is sometimes drowsy, sometimes delirious. The tongue is dry, and covered with a white fur; the lips also are dry, and are often loaded with black sordes."

^a *Diseases of the Skin*, Vol. i., p. 174.

I may recal the facts that there was no eruption of measles or variola on the face, neck, chest, or hands; no itching, no sore throat, no bronchial affection, no pain in the back; the absence of which signs rendered the case most perplexing, and difficult to be diagnosed.

Hæmoptysis, epistaxis, and hæmorrhage from the bowels may be present in both measles and variola, also hæmaturia; but the latter is very rare in measles, although it sometimes takes place. That the eruption was measles I have not the slightest hesitation in affirming, for the crescentic and annular patches of the rash with the intermediate unaffected portions of skin rendered it impossible, during the time I saw the patient, to be confounded with variola.

Hebra states that cases of variola hæmorrhagica, in which the only manifestation of the disease is the formation of hæmorrhagic spots, do not at all resemble those of ordinary small-pox; and, in fact, it is only during an epidemic that we can positively determine them to be due to the variolous poison. On *post-mortem* examination all the mucous membranes and the fibrous and serous tissues are found, like the skin, to be infiltrated with blood. Moreover the parenchymatous organs, such as the lungs, heart, liver, and spleen, are in a similar condition; indeed, the spleen is often so soft that it looks like nothing but a mass of blood enclosed in a fibrous capsule. There was no *post-mortem* examination made in this case, but I have little doubt that this extravasation of blood into the different tissues and viscera of the body described by Hebra would have been found if such examination had been made.

My opinion for stating that the variolous poison was present with the measles (malignant or otherwise), is on account of the conjunctival ecchymosis, the exanthematous rash, like scarlatina, and the hæmatemesis. For I look upon the ecchymosis as peculiarly diagnostic of small-pox, and also of a very dangerous form of the disease; besides, hæmatemesis and effusion of blood under the conjunctiva have never, to my knowledge, been seen in measles. These are my chief reasons for believing that the measles were associated with variola. I am aware that in these cases of hæmorrhage or black small-pox, that circular tubercles about the fingers and toes, with enlargement of the inguinal, axillary, or cervical glands are additional symptoms, but they were absent in this patient.

A very remarkable occurrence in these cases of hæmorrhagic variola is the calm, intelligent manner of the patient up to the last

moment; this, I believe, is the rule, and it was found existing in this case also, as Mr. Jones states in his notes, "he was perfectly collected until his death." The temperature is also said to be not so high as in the confluent form, seldom as high as 105° , whilst in the latter (confluent) it has been observed from 106° to even 108.4° . Those attacked die early, rarely living beyond the 8th day. A few distinct vesicles may arise—in this case there were none—but they become rapidly black, and never purulent.

I append the pulse and temperature, in a tabular form, for the purpose of noticing the fluctuations of each:—

	PULSE.		TEMPERATURE.	
	Morning.	Evening.	Morning.	Evening.
5th March (2nd day of illness)	—	87	—	99°
6th " (3rd ")	93	84	99.75°	99.75°
7th " (4th ")	90	96	102.75°	103.75°
8th " (5th ")	102	102	103.50°	104.6°
9th " (6th ")	122	124	101.2°	102.1°

Death took place on the morning of the 7th day.

ART. XIV.—*The Treatment of Enteric or Typhoid Fever.* By JAMES LITTLE, M.D., Univ. Edin., M.R.I.A.; one of the Physicians to the Adelaide Hospital; Lecturer on the Practice of Medicine in the Ledwich School of Medicine; and an Examiner in Practice of Medicine and in Clinical Medicine to the College of Physicians.

DURING the past three weeks an unusually large number of persons suffering from enteric or typhoid fever have presented themselves at the Adelaide Hospital, they have not come from any single neighbourhood but from various localities in and around Dublin. I therefore think the disease just now prevails somewhat extensively, and I am anxious to draw attention to its treatment, because even the last editions of the standard text-books do not, in my opinion, tell all that in the present state of our art we can do for the disease, in one important matter, as I conceive, the advice given in all of them is injudicious, a valuable means of treatment is not mentioned at all and the indications for the employment of exceptional measures are not stated with sufficient precision. Several of those I adopt are, I know, daily prescribed by other physicians in this city, and most of them I have copied from the practice or tried on the recommendation

of some one else; but the opinions I now express regarding them are founded on their observed effects in my own cases, and the justification of my present description of them arises from the circumstance that they are not generally known, or, as I believe, sufficiently valued.

Next to early confinement to bed, which perhaps more than anything else lessens the severity and risk of the fever, I rank the rigid exclusion of animal broths and jellies from the food, as tending to keep the disease mild. On this point I find myself quite at variance with the text-books in which such articles as beef-tea and Liebig's essence of meat are recommended. Dr. Hudson in his *Lectures on Fever* insists on the liability of all kinds of broths to increase the diarrhœa, and I cannot but attach the utmost importance to this matter, as I have repeatedly seen a patient who was passing four to six stools on a milk diet have eighteen or twenty during the day and night after he had taken beef-tea. Milk should be the chief article of diet in enteric fever. Thirsty patients sometimes object to its mawkish taste, and I generally, therefore, add ice to it, and a little lime water in cases in which it returns curdled. Junket or rennetted milk given before it has separated into whey and curd, rice milk, custard, baked custard in small quantity, rusks and hot milk, and blanc-mange, generally afford sufficiently varied ways of giving milk. Freshly made chicken jelly is less liable than beef-tea to increase the abdominal symptoms, and I use it in those cases in which milk, even with lime water, disagrees, but in my experience such an occurrence is very rare, and when encountered is usually in a person chronically dyspeptic. For years I have made the administration of two or three cups of really good tea or coffee between daybreak and two in the afternoon a regular part of the treatment of every case of fever, unless there was in the state of the nervous system some evident contra-indication. I did this in consequence of the well-known observations of Dr. Parkes on the effect of coffee in increasing the elimination of urea in fever, and I am satisfied that both it and tea lessen drowsiness and prostration, and increase the secretion of urine; once or twice in the day they may be given, poured upon a well-whisked egg, and thereby an additional means of nourishing the patient is obtained. I agree with those who consider alcoholic stimulants in any quantity seldom needed in enteric fever.

Cold baths I have systematically employed since Dr. Cuming in his *Report on Medicine* in this Journal (May, 1869), drew attention

to their use in enteric fever. I have not given them at such short intervals as recommended by Jürgensen and by Bartels and Liebermeister; three, or at most four are given in the twenty-four hours. In the hospital we have full-sized baths, which run on castors, one of these is brought to the bedside, half full of water at 75° Fahr., and if the temperature of the patient exceed 102° Fahr., he is lifted out and laid in the bath; he is kept in it for a period varying from five to fifteen minutes, and then quickly dried and put back to bed. The bath is usually most grateful to the patients; they often say it makes them feel strong, and after the evening bath they commonly fall into a sound sleep. I have used it with manifest benefit where cooing and wheezing râles existed in the chest, and where deficiency in the percussion resonance posteriorly and mucocrepitus indicated postural stasis in the lungs; but I have never used it when there was hæmorrhage from the bowels, or such pain as to justify the fear that peritonitis existed. There is sometimes slight chilliness in the extremities after it, and a little shivering. This indicates that the bath should not be a prolonged one, but does not forbid its use. Twice, however, I have considered it unsafe to continue the baths, once because a marked shivering followed, and once because the patient was alarmed by it. In cases in which the disease is running a mild course I only give one bath in the day, at the height of the usual evening paroxysm of fever.

By a dietary such as I have described, and by the systematic employment of the baths, I am convinced the severity and danger of enteric fever are greatly diminished, and the occurrence of any of the serious accidents incidental to the complaint rendered very rare; but I do not believe that its duration is appreciably shortened.

Besides these means there are others which I have found of material benefit when certain conditions were present.

When during the first eight days the face is flushed and there is headache, a high temperature and a thickly coated tongue, and when the evacuations, three or four in the twenty-four hours, are neither very large nor very liquid, a dose of calomel, from four to six grains, perceptibly lessens the heaviness of the fever. I have sometimes given the calomel a second time, after an interval of a day or two; but never oftener; it increases the evacuations during the twelve hours after its administration, but never in my experience sets up troublesome purging.

In enteric fever it is not uncommon to find a patient lying on his back, perceptibly impeded in his breathing, his abdomen tumid and

projecting, but not markedly tender, and on inquiry it will be found either that the bowels have not acted for twelve hours, or that though the stools are frequent, only a very little fœcal matter with wind passes each time. In such a case great relief is obtained by giving a draught containing two drachms of castor oil, with one or two of turpentine. If for any reason the physician hesitates in ordering this, he may have a warm water enema administered, but the draught relieves much more completely.

In my experience poultices and fomentations have not proved so useful as their constant recommendation by authors led me to expect; their weight, and the bandaging necessary to keep them properly applied render them anything but grateful to most patients, and I now seldom employ them except when the abdomen is tympanitic, in which case the turpentine stupe is of undoubted utility.

Since I have kept my patients rigidly to the diet I have mentioned, I have not often found it necessary to give medicines to check looseness of the bowels; three or four actions in the twenty-four hours seldom do any harm; when it is necessary to interfere I have found by far the most useful remedy a pill, the use of which Dr. Hudson some years ago suggested to me, it contains one-sixth of a grain of carbolic acid, one-sixth of a grain of opium, and three grains of bismuth. This combination I have found so admirable for moderating diarrhœa and tympanitis that I have seldom employed any other; two, three, or four pills may be given during the day and night; of other remedies sulphuric acid is, I think, the best.

Hæmorrhage from the bowels is also rare when milk diet and cold baths are employed; when it occurs, gallic acid, a scruple every second or third hour, and turpentine were the remedies upon which, until lately, I relied; in consequence, however, of the power of arresting hæmorrhage, which ergotin has recently been shown to possess, when administered hypodermically,* I some weeks ago made up my mind to try it in the hæmorrhage of typhoid, as soon as a case presented itself, and as far as the experience of a single one goes, I have reason to be well satisfied with the experiment. On the morning of the fourteenth day of enteric fever, blood appeared in the discharges

* I have found the hypodermic injection of ergotin check violent hæmoptysis in acute phthisis, and arrest rather profuse hæmaturia in a case which is probably one of malignant disease of the kidney. I use five grains suspended in twenty or thirty minims of distilled water; severe pain and swelling have invariably followed the injection, and I now always throw it into the loose cellular tissue of the buttock, where it produces less suffering than if injected into the arm.

of a patient now under my care in the Adelaide Hospital; when I visited her at noon I found more than a pint of clots had come away, and while I was in the ward more than half a pint of pure blood, partly clotted and partly liquid was passed, at the same time the features became sunken, and the extremities cold. I then injected five grains of ergotin, and repeated the injection at three o'clock. Within half an hour of the first injection about three ounces of blood escaped from the bowel, but none came again till four o'clock the following morning, when half a pint of clots escaped, and a third injection was used; the only other treatment employed during that day and night was the application over the ilio-cæcal region of an ice bag. There was no further discharge of pure blood; the three or four next stools were, however, blackish, and as a precaution three twenty grain doses of gallic acid were given on each of the two following days.

There is a group of nervous phenomena sometimes present in typhoid fever, for which, I believe, the remedy is a full dose of quinine. I have seen similar symptoms occurring at the commencement of cerebro-spinal fever relieved by quinine, and was led in consequence to try it in the enteric disease. A delicate lad of fourteen, in whose case I had the advantage of Dr. Hudson's counsel, was attacked with enteric fever when recovering from a pulmonary affection; most of the febrile phenomena, though quite characteristic, were not severe, but in the beginning of the third week, although the bowels were free and the urine abundant, he became drowsy, and his breathing and pulse of the character which indicates cerebral oppression, it became difficult to arouse him sufficiently to swallow, and for several days he did not speak; a blister was applied to the nape of the neck, and turpentine with ether, and a moderate allowance of brandy given, but without marked effect on the cerebral symptoms; after these had persisted five days, he was in the following condition:—He lay as if heavily asleep, with difficulty could he be roused to swallow; he never spoke, and when I succeeded in getting him to put out his tongue he did not draw it back again till again aroused; he only indicated by restlessness when his bladder and bowels were going to act; his morning temperature was 104°, his respirations 52, and his pulse exactly equalled the respirations, it was also 52, and had a distinct intermission every fifteen or twenty beats; six grains of quinine were now given, and the dose repeated in twelve hours. The following morning the temperature was 101°, the respirations had

fallen to 40, and the pulse risen to 68 without intermissions. A third and fourth dose of quinine were given, and the turpentine continued, and, though slowly, the cerebral symptoms disappeared, and the lad made a good recovery. I think it is probable these symptoms indicate venous stasis or serous effusion at the base of the brain; but whether this be so or not, it is necessary carefully to distinguish the condition, to which I now allude, from the cerebral oppression due to deficient secretion of urine, the most common cause of stupor in fever, and one which, of course, requires a very different treatment.

The opinions I have formed as to the best way of combatting other dangers in enteric fever, do not materially differ from those to be found in the text-books, and I will, therefore, merely state the measures which, out of the various ones enumerated in these works, I have found the best.

For delirium and wakefulness, in the early stage, leeches to the temples, and, later, provided the urine be fairly abundant, a dose of four, five, or six grains of Dover's powder will be found superior to any of the hypnotics recently introduced; headache may sometimes be temporarily relieved by the application of the vapour of bisulphide of carbon, as suggested for neuralgic headache, by the late Dr. Kenyon, but cutting the hair and leeching are the remedies for severe headache.

Nausea and persistent retching in the early days may usually be relieved by an emetic of ipecacuanha, and, later on in the fever, by ice and by draughts containing ten grains of bicarbonate of soda, ten grains carbonate of bismuth, and four minims of prussic acid.

Scantiness of urine requires dry cupping of the loins, effectually performed, and the internal use of the salts of potash and the spirit of nitrous ether. The former of these measures is not, I think, adopted in fever as often as with advantage it might be; when actual stupor with suppression of urine occurs it is the remedy on which most physicians rely, but I am sure that it is often desirable to resort to it when there is merely drowsiness and confusion of mind coincident with a scanty renal secretion. A stout young woman, for whom, under these circumstances, I lately prescribed cupping, told me the next morning she felt "altogether different" after the cupping, there was consciousness of amendment, a symptom of great prognostic value in fever.

Indications of more or less pulmonary congestion are very usual in enteric fever, and often do not call for interference. When there

are cooing and wheezing râles, and the sleep is broken by fits of coughing a turpentine stupe at bed-time is usually sufficient to relieve. When there is any deficiency of resonance posteriorly I attach most importance to the precaution on which I first heard Dr. Stokes insist, of not allowing the patient to remain on his back, but keeping him alternately on his right and left side, supported if need be by pillows. For graver chest symptoms, turpentine, ether, and brandy are the only remedies on which, I think, any reliance can be placed.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

The Medical Jurisprudence of Insanity. By J. H. BALFOUR BROWNE, of the Middle Temple, Barrister-at-law. London: J. & A. Churchill. 1871. 8vo, pp. xvii., 341.

AMONG the subjects with which forensic medicine is conversant, there is none which can be deemed more important, or better entitled to studious attention, than insanity in its many varieties and developments. With all the other matters in which legal and medical science are brought into contact this one of mental condition may be introduced as a complication, by which the original question, whatever it may have been, will be so influenced that the ultimate result may be diametrically opposed to that which would have been produced if it were not for the presence of this disturbing force. Responsibility for crime, capacity for contracting, and ability to make valid testamentary disposition, are all dependent on this condition, that mental unsoundness does not exist. And thus, in a great variety of circumstances, in opposite and diversified states, and in the most unexpected positions, we may be surprised by the unwelcome cropping out of this perplexity, and be compelled to engage in inquiries, and to address ourselves to the solution of obscure problems, for which previous studies might have very inadequately, if at all, prepared us.

We took up Mr Browne's book with some misgivings; our recollections of Chitty's Treatise on Medical Jurisprudence (Lond. 1834), had not prepossessed us in favour of another book on a medical subject, written by an author not of the medical profession, but we must admit that, in the present case, we have been agreeably disappointed, on the perusal, by the learning and judgment of the writer. We do not, indeed, profess to agree with all his opinions, but they are well entitled to consideration, and if he has not solved all the problems connected with his subject, he has, at least, contributed much assistance to the student and the inquirer into the various forms of insanity.

This work will not, indeed, displace those of Pritchard, Ray, and J. F. Duncan, or cause them to be regarded as superfluous in the philosophical physician's library, but for those whose concern is more with the juridical, than with the merely medical part of the subject, it may well supply the place of any other. There are a few passages only, in which we remark a flippancy of style that is foreign to the character of a didactic work, but in which many writers on mental diseases, as they are called, have thought themselves at liberty to indulge. This is, however, but a trifling blemish, that detracts nothing from the real value of the book, and may readily be removed in future editions.

It is found exceedingly difficult to form any adequate definition of insanity; that is, which shall not err either by admitting, or excluding. It is, at least, equally difficult to form a satisfactory pathology capable of being adjusted to the various observed conditions. From morbid anatomy we have not much assistance; we may deduce more from analogies with more obviously bodily diseases. The state of the corporeal parts with which the faculties of the will and judgment, and intellectual powers are probably connected, as the various senses are with their several organs, may be such that either with or without cognizable appreciable local changes, deranged action may result, just as what are termed functional and organic lesions occur in other systems. There is, at least, a high degree of probability that what is styled mental disease is connected as a consequence with bodily disease, however we may be unable to trace out this relation of cause and effect demonstratively. Such an investigation, though highly interesting to the physician, would be foreign to our purpose here, which is only to give some account of Mr. Balfour Browne's book.

There is no scarcity of medical books on the subject of insanity. They are rather confusing by their number, and there are also many purely legal works in which are collected together the laws relating to the insane, and the adjudicated cases, both civil and criminal, in which the questions of responsibility for acts committed, and capacity for contracting and disposing, have been amply discussed, so that all available information might readily be brought to converge on either division of the subject. But we do not recollect any instance, before the present work, of a barrister having attempted, without the assistance of any medical *collaborateur*, to grasp the entire of so vast a theme as "The Medical Jurisprudence of Insanity."

The work is divided into thirty-three chapters, in which the author brings clearly before his reader—Unsoundness of Mind (Chap. III.); The Causes of Insanity (Chap. II.); Lunacy and Limited Responsibility (Chap. I.); The Pathology and Symptoms of Mania (Chap. V.); The Varieties of Intellectual, Moral, and Partial Moral Mania (Chaps. VI., VII., VIII., IX., X.); Amentia (Chap. IV.); Dementia (Chap. XI.); Epilepsy (Chap. XII.); Somnambulism (Chap. XIII.); Drunkenness (Chap. XIV.); Acute Delirious Mania (Chaps. XVI., XVII.); Aphasia (Chap. XV.); Feigned Insanity (Chap. XVIII.); Concealed Insanity (Chap. XIX.); Lucid Intervals (Chap. XX.) In Chapter XXI. he discusses whether, and how far the evidence of insane persons may be admissible. The Prognosis of Insanity is the subject of Chap. XXII; and the book concludes with Chapter XXIII., on the examination of persons supposed to be of unsound mind. A good alphabetical index facilitates reference to every part, and there is prefixed another alphabetical table of the various cases cited in the book. Chapters XVIII., XIX., XX., and XXIII. are, in our opinion, the best parts, and might advantageously be republished as a distinct work.

The author has brought to the composition of his book a surprising amount of both legal research and of acquaintance with the previous writings of medical and other authors on this subject. He has digested and arranged the matter very perspicuously, and has produced a work, which, while it cannot but be highly useful to the medical practitioner, will be of still higher value, indeed, may, without making any exaggerated estimate, be deemed indispensable to barristers and magistrates, as furnishing, within a single volume, what is most necessary for them to know on the subject of insanity and its legal relations.

Sanity of mind, and, consequently, responsibility and capacity, should be presumed, until the contrary is proved. But it very often has happened that mere conjecture, or opinion is accepted as such proof, and that even in cases where opposite views were taken by different medical witnesses. How are such conflicts of testimony to be reconciled? As to matters of fact there should be a complete agreement between witnesses having equal means of observing, and who have availed themselves equally of the opportunity for making such observation; but as to the inferences to be deduced from the observed facts, it is scarcely to be expected that an absolute unanimity should result. The judges in our Law and Equity Courts,

though accustomed to examine and estimate evidence, cannot always agree with each other as to the decision. It is, therefore, unfair to object to the disagreement of medical witnesses, as if it were something peculiar to their profession, and that no such divergence of opinion existed among others, in similar or analogous circumstances. In giving evidence on the question of sanity or insanity, capacity or incapacity, responsibility or irresponsibility, the medical witness should be especially careful to have reasons for his opinion, and to be able to state those reasons with promptitude and perspicuity.

If we review the decisions that are on record, we sometimes find, that whether as a consequence of contradictory evidence, or of some other less obvious cause, there is a discordance that is very difficult to be explained away, or reconciled with any known principle. The Windham case appears, in principle, to be contradicted by the later one of Sir A. Fitzgerald, and we may regret that we have not, as the old Romans had, a power in a court of Equity to interdict a notorious prodigal or waster of his estate, without the difficult, tedious, and expensive process of a commission to inquire whether he be or be not of sound mind and capable of managing his affairs. Nor is the state of matters regarding the plea of insanity in criminal cases more satisfactory. The plea itself is like that of "alibi," an excellent defence if only it can be proved. But there is much that is vague, illogical, and even untrue, in what is offered, and too readily accepted as conclusive evidence in its favour. Is the commission of a crime to be accepted as such proof? Yet, instances could easily be adduced in which the crime itself has been accepted as its own excuse; or in which, after such a plea being rejected, after full inquiry by a competent court of justice, it has subsequently been accepted by a secret irresponsible tribunal, without any testimony on oath, and without the evidences, such as they might be, being tested by any cross-examination as to the value of the opinions, on the credit of which the decision of the tribunal is sought to be reversed or annulled.

It has been the fashion of recent times to disparage the knowledge and the opinions of our ancestors regarding the insane. It is true that their theoretical medicine was very faulty, and encumbered with hypotheses. Their observing and reasoning powers were, however, good, and on this subject of insanity they were not without some just and correct opinions based on observed facts. Thus they had remarked the congenital deficiency that constitutes

idiocy—the temporary delirium of some diseases; the furious and the desponding mania; the imbecility of old age, and, occasionally, of chronic disease; and the periodical paroxysms—between which were remissions (the *lucid intervals* of the law-writers) which they supposed to be connected with lunar influence, and whence the term *lunatic* has been adopted. They were ignorant of the pernicious subtleties by which, in this “patent age of new inventions,” the boundaries of crime and disease are effaced, guilt is confounded with the consequences of organization, and we are required to believe, without a tittle of proof, that because an impulse has been uncontrolled, it should, therefore, be regarded as incontrollable. Such principles applied with logical consistency would destroy the distinction between virtue and vice, abolish moral responsibility, and establish, in its stead, a doctrine of physical necessity, more injurious to the interests of human society than was that of inevitable destiny in the old heathen world.

As we purpose to return to the subject, we postpone, until a future occasion, the consideration of some passages on which we had intended to make remarks, but the discussion of which, at present, would compel us to far exceed our limits.

Recueil d'Ophthalmologie. Du Dr. X. GALEZOWSKI. 1re Année.
Paris: 1871.

A Collection of Papers on Ophthalmological Subjects. By Dr. X. GALEZOWSKI. 1st Year. Paris: 1871.

WE could wish to see the practice more frequently adopted of selecting and re-publishing, in a collected form, those papers and communications on particular subjects, by authors of distinction, which have from time to time appeared in the several journals, or been read before the various societies.

Were such a practice more general it would save an enormous amount of time and trouble in the way of reference. Moreover, it frequently happens, especially if any time has elapsed, that it is impossible to procure the particular number of the journal or transactions of the Society in which the paper or communication sought for originally appeared.

Dr. Galezowski's *Recueil d'Ophthalmologie* affords a striking example of the advantages of such re-publications, for not only are

all the very excellent papers on subjects of special interest, but most of them having been originally published in different journals, any person anxious to consult them might be put to serious inconvenience and trouble were it not for the present admirable and judicious collection.

The subjects of the several papers are as follows:—

I. On Optic Neuritis and Perineuritis, and their relations with Cerebral Affections.

II. Some Remarks on Operations for Cataract.

III. Linear Extraction.

IV. Endemic Hemeralopia and its treatment by Esérine (Calabarin).

V. Essay on Syphilitic Amblyopia and Amaurosis.

The first of these papers is to a great extent a re-statement and elaboration of the views already expressed by the author in his well-known work on *The Changes of the Optic Nerve in Cerebral Diseases*.^a

A very special interest attaches itself to this subject on account of the recent appearance of Dr. Clifford Allbutt's work: *On the Use of the Ophthalmoscope in Diseases of the Nervous System and of the Kidneys*. Dr. Allbutt bears testimony to the correctness and importance of Galezowski's discovery as to the vascular supply of central organs of vision and optic disk. "Since I became acquainted with Galezowski's views in 1866," he writes, "I have gone carefully into this important question, and I have satisfied myself by means of many dissections, microscopic and other, of injected parts, and also by observation of the behaviour of the disk and retina in disease, that Galezowski in this important matter is mainly right."

In speaking of perineuritis Dr. Galezowski states that he is opposed to recognizing that form of neuro-retinitis originally described by von Graefe, and which is characterized by great swelling and congestion of the disk (Stauungs' papilla). The chief distinction of this form of optic neuritis, as pointed out by von Graefe, is, that the morbid changes do not extend beyond the lamina cribrosa. "It appears to me," says Dr. Galezowski, "very difficult to admit that an inflammation of the optic nerve can be limited only to the surface of the papilla, without the optic nerve itself being affected to a greater or less extent posteriorly." At first sight there certainly seems to be sound reason in this remark,

^a Etudes Ophthalmoscopique sur les Altérations du Nerf Optique et sur les Maladies Cérébrales dont elles dépendent. Paris: 1866.

especially when we consider that the “tendency to propagate itself along the sheath of a nerve is very characteristic of neuritis, and it is by means of this property that neuritis occurring in any encephalic portion of the optic nerves sooner or later presents itself in the eye.”^a

Von Graefe, however, in his paper reports two cases, in both of which he states emphatically that the pathological changes were “*solely confined to the papilla and adjoining portions of the retina; the extra-ocular portion of the optic nerve presented no anomaly.*”^b The microscopical examinations were made by Professors Virchow and Schweigger.

Some recent researches by Dr. Hermann Pagenstecher seem also corroborative of this statement.^c

On the whole, however, we are strongly disposed to agree with Dr. Allbutt that many of the cases described as “optic neuritis” are really nothing more than cases of great congestion of the optic nerve. “I have described this change,” he says, “as ischemia papilla, and it is not only incorrect, but very misleading, to call this result ‘optic neuritis.’ Injury to tissue is not the starting-point of the disturbance, though, of course, a secondary neuritic process, with increased proliferation of the connective elements, may be set up in consequence of the pressure.”^d

As to the mode in which intra-cranial tumours may produce optic neuritis, Dr. Galezowski rejects the theory of von Graefe, that it is due to increase of the intra-cranial pressure; for, he argues, if such were the case we should find it also present in cases of hydrocephalus, cerebral congestion, &c.; whereas, what we do find in these cases generally is progressive atrophy.

As regards hydrocephalus, however, it should be remembered that it occurs at a period when the cranial bones have not yet united, and consequently with every increase of pressure there is a corresponding decrease of resistance. Curiously enough, Dr. Galezowski himself makes use of this very argument in the case of a boy aged fourteen, who had a tumour of the cerebellum with optic neuritis, and in whom *the cranial bones were not united*, to show that in this case the theory of increased intra-cranial pressure could not be adduced.

^a Dr. Clifford Allbutt in lib. cit., p. 59.

^b Archiv. für Ophthalmologie. Bd. vii. Heft 2, p. 58.

^c Royal London Ophth. Hosp. Reports, Vol. vii. Pt. ii., p. 126. Nov., 1871.

^d Loc. cit., p. 59.

On the other hand, Dr. Allbutt, speaking of hydrocephalus, says:—"Ischæmia papilla seems in many cases to be the earliest change, as I have certainly noticed it in early cases more than once. Herein I am obliged to differ from Graefe, who has only found simple white atrophy in cases of hydrocephalic amaurosis. It is likely that Graefe, however, does not see hydrocephalic children until the mischief is old and the loss of sight obvious."^a

Dr. Galezowski states it as his opinion that a cerebral tumour, in order to produce optic neuritis, must be in the neighbourhood of the central organs of vision:—

"The vicinity of the central organs of vision is, in my opinion, indispensable to the production of optic neuritis and loss of sight by cerebral tumours; unless there be vicinity the disease may attack some other nerve, and the nerves being affected, paralysis of this or some other organ or muscle will follow, the sight remaining perfect. It is thus that tumours situated on the surface of the hemispheres, the pons varolii, the central or posterior parts of the cerebellum, &c., rarely produce optic neuritis and amaurosis. On the other hand, the same changes taking place in the regions indicated (anterior lobe, pituitary gland and chiasma, posterior lobe, cerebellum, *circa cerebelli*, fourth ventricle, and thalami optici) cannot occur without affecting the organ of vision."

The paper on Endemic Hemeralopia possesses considerable interest. The author has met with several cases of this curious disorder, especially amongst soldiers. In many of these cases he has noticed a remarkable ophthalmoscopic appearance, consisting in partial obliteration of the arteries of the retina, for a distance equal to or double the diameter of the papilla. "It appeared to me," says Dr. Galezowski, "that this was due to a species of contraction of the vascular walls, which, in my opinion, serves best to explain this form of retinal anæsthesia which manifests itself every evening."

"Relying upon these facts," he adds, "and the explanation which I have given, I decided in these cases to make use of a collyrium, containing *ésérine* (an alcaloid, the active principle of Calabar bean), $\frac{2}{5}$ of a grain, water 3 drachms, hoping by this means to dilate the vessels, overcome their spasm, and restore the circulation. Experience has fully confirmed the justness of my observation, and at the end of the third or fourth day of the instillation of the collyrium of *ésérine*, the hemeralopia had totally disappeared, and the circulation became normal throughout the retina."

^a Loc. cit., p. 112.

The beneficial effects of this treatment were further tested and confirmed by a friend of the author's, M. Perréon, a surgeon-major in the army.

The concluding paper on Syphilitic Amblyopia and Amaurosis contains a careful record and summary of the author's latest observations on this most important subject. Some of his conclusions differ in a measure from the more generally received opinions; for instance, in speaking of syphilitic retinitis, after alluding to the pretty general opinion that it is the choroid which first becomes affected, the retinal mischief being subsequent, he adds—"My own researches enable me to state that, although in the majority of cases this opinion is perfectly correct, there are, nevertheless, a certain number of cases of syphilitic retinitis, apoplectic or serous, devoid of any change either in the choroid or vitreous. In other cases retinitis is developed from the first, and the choroid does not become affected till a much later period and in a secondary manner."

Again, he is of opinion "that the optic nerve may be affected by the syphilitic virus in a completely isolated manner, and independently of any other change in the internal membranes of the eye or brain."

After the observation and comparison of more than fifty cases of congenital retinitis pigmentosa, and more than seventy cases of syphilitic choroido-retinitis, he has arrived at the conclusion that these diseases are identical, and have the same origin.

Partial colour blindness is a phenomenon he has frequently observed in cases of syphilitic choroido-retinitis, and he claims the right of being the first to point out the almost constant occurrence of hemeralopia in this disease, more especially in its advanced stage. This symptom he has observed to be as constant as in cases of retinitis pigmentosa.

In concluding his valuable and interesting paper, Dr. Galezowski sums up the results of his observations in the following aphorisms:—

"1. Syphilitic retinitis and neuritis may be present without any change in the choroid, and most frequently in the form of apoplectic or serous retinitis. However, these cases are exceptional.

"2. Syphilitic retinitis presents no pathognomonic signs, which can distinguish it from any other form of retinitis.

"3. If the retinitis or neuritis be accompanied by iritis or choroiditis, with or without opacities in the vitreous, there is then *no* doubt that the

affection is syphilitic. Experience has proved to me that there is no other affection, except glaucoma, which is at the same time subject to retinal apoplexies and iritis and choroiditis.

"4. The disorders of the chromatic faculty are constant in both these forms of disease, and especially in the optic neuritis.

"5. The most efficacious treatment in these diseases is the iodide of potassium and the perchloride of mercury, gradually increased to very large doses.

"6. Syphilitic choroiditis is one of the most usual forms of syphilitic amblyopia and amaurosis. The signs of this form of choroiditis are very characteristic, and I should say even pathognomonic of syphilis. They are—(1) Blindness or loss of vision occurring in paroxysms, frequently at very long intervals; (2) a cloud resembling a spider's web floating constantly before the eyes; (3) very frequently photopsies; (4) photophobia; (5) hemeralopia at a more advanced period of the malady; (6) central vision remaining perfect for a long time with contraction of the peripheral field of vision; (7) disc cloudy; (8) retinitis pigmentosa becomes apparent at a more advanced period; (9) atrophy of the central vessels of the papilla, with preservation of its normal colour, due to the cerebral or nutritive vessels of the optic nerve.

"7. Retinitis pigmentosa very frequently follows as a consequence of syphilitic choroiditis.

"8. The syphilitic pigment spots follow the course of the retinal vessels; but they form besides circular masses, cercinated, resembling circles of herpes cercinatus.

"9. Acquired syphilitic retinitis pigmentosa does not differ from congenital retinitis pigmentosa, especially that which has been considered due to consanguinity of parents, except in the circular form of the pigment spots.

"10. Congenital retinitis pigmentosa is an hereditary syphilitic affection.

"11. Congenital retinitis pigmentosa should be subjected to mercurial or iodine treatment from infancy. A certain age being passed, the disease can no longer be arrested; it becomes progressive, and at a more or less advanced age destroys the sight.

"12. Children born of syphilitic parents ought from their birth to undergo an ophthalmoscopic examination, and the retinitis once recognized should be treated as above mentioned."

Traité des Maladies des Yeux. Seconde Partie. Par X. GALEZOWSKI, Docteur en Médecine de la Faculté de Paris; Lauréat de la même Faculté; Professeur Libre d'Ophthalmologie à l'École Pratique de la Faculté de Paris; Chevalier de l'ordre de Charles III. Paris: J. B. Baillière et Fils. 1872. 8vo, pp. 423.

A Treatise on the Diseases of the Eye. Second Part. By X. GALEZOWSKI, Doctor of Medicine of the Faculty of Paris, &c.

THE first part of this treatise has already been favourably noticed in this journal; the second part, which completes the work, fully sustains the admirable character of its predecessor.

The subjects treated in the second part are arranged as follows:—

The ophthalmoscope; the vitreous humour; the optic nerve; the retina; the choroid; refraction and accommodation; the muscles of the eye; the orbit; legal and hygienic medicine.

Time will only allow of our taking a very cursory glance at the author's treatment of these various subjects, and in doing so our desire is not to criticise, but rather to present, as far as the space accorded to us will permit, the author's views regarding questions of special interest.

The two first chapters contain a short history of the introduction of the ophthalmoscope, and a description of the various modifications of that instrument, together with some useful and practical hints with reference to its manipulation.

The diseases of the vitreous humour are next considered. We may mention that our author, while admitting the possible occurrence, though very rarely, of hyalitis—*i.e.*, uncomplicated inflammation of the *hyaloid membrane*, expresses himself as strongly opposed to the opinion held by Wecker and others, that it consists not in inflammation of the *hyaloid membrane*, but inflammation of the vitreous humour itself.

"As to the vitreous humour," says Dr. Galezowski, "the changes we are at present familiar with in it are merely secondary, and result from inflammation of the retina or choroid, hæmorrhages from these membranes, and other causes."

The next section, which treats of the optic nerve, needs little more than passing mention, as the principal points of interest are identical with those contained in the paper on optic neuritis, &c.,

already noticed in the preceding review of the author's *Recueil d'Ophthalmologie*. In speaking of the possibility of mistaking an engorgement of the capillary vessels of the papilla for a hæmorrhage, he suggests the simple expedient of compressing the globe with the finger—if the spot be due to congestion it will disappear, but if it be a hæmorrhage it will remain unchanged.

The chapter on atrophy of the optic nerve is full of interest, and contains a very complete and careful analysis of the several causes which lead to atrophy.

In the chapter descriptive of the anatomy of the retina we are surprised to find no mention of Professor Krause's work* on *The Fenestrated Membrane of the Retina*, which is undoubtedly one of the most important contributions on the anatomy of this part of the eye, which has appeared within the last few years.

Dr. Galezowski looks upon retinitis albumenurica as due to two different causes—1st. A morbid state of the blood; 2nd. A diseased condition of the walls of the blood-vessels.

“When it is merely the composition of the blood which is altered, the latter, having become more fluid, transudes through the walls of the vessels, and produces serous infiltrations, which, in their turn, are succeeded by inflammatory processes. The retinal change which follows is, relatively speaking, not serious—the exudations become absorbed, and the sight returns. Such are the cases of albumenuric retinitis met with in parturient women.

“In the second variety of albumenuric retinitis the walls of the vessels are affected; they undergo atheromatous or fatty degeneration, and at the same time the blood becomes not only hydræmic, but also contains urea. The ruptures of the vessels are accordingly more numerous, and the blood itself, containing morbid and irritating elements, being once extravasated, becomes a new source of irritation and consecutive degeneration. This constitutes the serious form of albumenuric retinitis.”

Our author appears to have met with an unusually large number of cases of partial embolism of the arteria centralis retinæ. In the majority of these cases it was only the peripheral portion of the field of vision which was affected. In the case of one patient there was total blindness for twenty-four hours, but eventually one-half of the field of vision recovered: in another case visual acuteness was preserved, whilst the peripheral field of vision was contracted concentrically.

* Die Membrana Fenestrata der Retina. Von W. Krause. Leipzig, 1868.

The section which treats of the diseases of the choroid contains a very admirable chapter on glaucoma, in which Dr. Galezowski propounds the following theory as to its nature:—

“In my opinion glaucoma is an affection of the secretory nerves of the eye, as Donders has clearly demonstrated; sometimes, however, these nerves are only diseased at their termination in the ciliary processes, and then a very great increase of the rapidly formed secretion takes place; the sclerotic being dense, resisting, and not allowing of distension, the intra-ocular pressure pushes the iris, together with the crystalline lens, forwards, and the surface of the papilla backwards. Acute or chronic glaucoma is a venous glaucoma, for the ciliary processes are principally composed of veins, the inflammatory symptoms are merely due to a species of strangulation which the globe of the eye undergoes.

“On the contrary, when the vaso-motor nerves which supply the arteries are affected, the latter gradually distend, and as they chiefly occupy the posterior segment of the eye, it is especially in this region that the secretion will augment and press upon the papilla. This morbid pressure may remain localized at the posterior segment, provided the attachments of the lens are firm and resisting, and then no change will occur in the anterior portions of the eye. This latter form of glaucoma I term arterial glaucoma.”

The portion of the work which treats of refraction and accommodation is brief, yet at the same time both these subjects are very clearly and admirably dealt with.

Dr. Galezowski appears to have established one point beyond all dispute, and one which, as far as we are aware, has not been noticed by any previous writer on ophthalmology, and that is, that the mechanism of accommodation, the discovery of which is generally assigned to Thomas Young, was in reality discovered and accurately described by Descartes in 1664.^a The passages from Descartes' work quoted to show this are very curious and interesting. In one of them, after describing the structure of the eye, he goes on to speak of certain “small black filaments” which encircle the “crystalline humour;” these he looks upon as “small tendons,” by means of which its figure can be altered and rendered flatter or more convex, as occasion demands. Further on he adds:—“The change of figure which the crystalline humour undergoes acts in such a manner that the images of objects situated at different distances come to be painted distinctly on the fundus of the eye.”

^a Dioptrics, Chap. iii., sec. 5.

"It is evident," says Dr. Galezowski, "that the theory of Descartes needs no change, and that, without being aware of the existence of the ciliary muscle, he placed the contractile force in the *small back filaments* arising from the choroid and iris, and which are, as we now know, the fibres of the muscle of accommodation."

We cannot conclude without referring to the very important section on legal and hygienic ophthalmic medicine. If we do not mistake this is the first time the legal aspect has been prominently brought forward and systematized. The first chapter is devoted to the consideration of the ocular diseases which exempt the patient from military service.

The second is a most important chapter on simulated diseases, and the various methods for detecting them. Some curious and interesting examples which the author met with are recorded.

The third chapter treats of ocular diseases from the jurists' stand-point. It contains the record of a striking case in which a patient, who had double cataract, sued a doctor for malpractice. As the decision of the case necessarily involved scientific questions, the tribunal submitted them to experts. Dr. Galezowski having been called upon to give his opinion, gives a careful summary and analysis of the several points in question.

The section on hygiene contains some valuable rules and preventive measures for those who are engaged in trades and professions which are injurious to the eye.

In conclusion, we congratulate Dr. Galezowski on the very successful completion of his arduous task. We can safely recommend the treatise to the student, the general practitioner, and the specialist. Taken as a whole, it cannot fail to add largely to the author's reputation.

Spectrum Analysis in its Application to Terrestrial Substances and the Physical Constitution of the Heavenly Bodies. By Dr. H. SCHELLEN. Translated from the second and revised German edition by JANE and CAROLINE LASSELL. Edited, with Notes, by WILLIAM HUGGINS, LL.D., F.R.S. London: Longmans, Green & Co. 8vo, 662 pages.

WHEN certain substances are heated so highly as to become luminous, they evolve coloured light. If, for example, we heat a pure salt of potassium in a colourless hydrogen or coal gas flame,

the latter will acquire a violet hue. In this case the metallic compound is decomposed by the heated gas; the metal potassium is set free, but instantly burning in contact with air it evolves in so doing a characteristic violet light. In like manner sodium or its simple compounds, when heated to a luminous point, develops an intense yellow light. We can make use of these facts in chemical analysis: when we have a colourless flame changed to a deep yellow, by the introduction of a solid substance into it, then we know that the latter contains sodium, or soda. If, however, we heat to incandescence a mixture of one part of soda and two parts of potash a yellow light only will be evolved; for the faint violet colour of the incandescent potassium will be completely marked by the intense sodium yellow.

The spectroscope is an optical instrument which analyses the light evolved from all kinds of luminous bodies and spreads them out before the gaze of the analyst. If we examine through this instrument the light developed by a mixture of incandescent potassium and sodium, the colours are distinctly seen, because they are separated from each other.

The spectroscope was invented about a dozen years ago by Bunsen and Kirchhoff, and by means of it they very soon discovered the existence of the new elements, caesium and rubidium, and shortly afterwards, the element thallium was also discovered by spectrum analysis, by Mr. Crookes, of London. It is now largely used as a means of detecting minute quantities of bodies, for its analytical powers with regard to minuteness are really wonderful, and almost approach the infinite. The observations made with this instrument have been extended to the organic kingdoms of nature, and the spectra (images seen in the spectroscope) of blood, and other animal substance, afford reliable evidence of the existence of such bodies, when they are present in quantities so small as to elude ordinary observation.

The spectroscope has been with great success employed in detecting the composition of the sun and other heavenly bodies. In the solar-spectrum there are hundreds of dark lines, or spaces, the cause of which was inexplicable until after the invention of the spectroscope. The spectrum of each body, when seen in the spectroscope, is usually found to contain one or more bright and often brilliantly coloured lines or bands. Potassium has two characteristic lines, one red and the other violet. If while potassium in luminous vapour is producing its spectrum, a powerful white light is placed

behind it, the coloured bands will disappear, and dark lines will occupy their places. It has been discovered that the intense white light evolved from a solid incandescent is partly absorbed when it passes into luminous vapours or gases. The sodium spectrum consists of a dark space, with two bright yellow lines; but if a bright light from a solid body be caused to pass through the sodium vapour, a reversed spectrum of the latter will be produced—namely, a white ground with two dark lines corresponding to the previous coloured bands. It is believed that the dark spaces in the solar spectrum are produced by reversed spectra of certain elements which exist in the photosphere, or solar gaseous envelope. The intense white light evolved from the solid or liquid nucleus of the sun is partly absorbed by the incandescent gases surrounding it, and reversed spectra of the latter are produced when the solar light is examined by means of the spectroscope. The alkaline metals, hydrogen and other telluric elements have been detected by spectrum analysis in the sun and other heavenly bodies; and stellar chemistry is now being actively prosecuted by means of the spectrum analysis.

Dr. Schellen's book gives in a familiar manner a most interesting account of the present condition of spectrum analysis, and the work is enriched by numerous plates. It is written in a style which is calculated to teach its subject matters to persons of ordinary intelligence who have not hitherto studied optics as a branch of mathematical science. Ample justice is done in this book to the labours of British physicists, and the important paper on the cause of the interrupted spectra of gases read by Mr. G. J. Stoney, of Dublin, before the Royal Irish Academy, on the 9th January, 1871, is given in full as an appendix. We cordially recommend Dr. Schellen's work as a most delightful volume on one of the newest, most wonderful and most important departments of physical science.

Dr. Pereira's Elements of Materia Medica and Therapeutics.

Abridged and Edited by ROBERT BENTLEY, M.R.C.S., F.L.S., and THEOPHILUS REDWOOD, Ph.D., F.C.S. London: Longmans, Green, and Co. 1872. 8vo, pp. 1093.

THE fourth and last edition of Dr. Pereira's celebrated treatise, edited by Drs. A. S. Taylor and G. O. Rees, appeared in 1854. It has always maintained its high reputation as *the* representative English work on Pharmacology, but its very completeness and the

expense attending it rendered it comparatively inaccessible to students, while, of necessity, in some respects, and more particularly in the sphere of chemistry, its teaching lags behind that of the present day. Accordingly an abridgment of Pereira's work, adapted to the first edition of the British Pharmacopœia, was published in 1865, under the supervision of Messrs. Farre, Bentley, and Warrington. The present goodly volume, which is excellently printed and clearly arranged, is brought out in accordance with the last edition of the Pharmacopœia, and includes not only the officinal medicines, but also such other remedies as have been recently introduced. The classification of the chemical compounds is to some extent altered, and the modern notation is exclusively employed in the explanation of reactions. In the vegetable *Materia Medica* much new matter not contained in the previous abridgment is introduced, the marks of careful revision are apparent, and additional illustrations are given. The merits of Dr. Pereira's splendid work are so universally acknowledged that any volume based upon it is sure to find acceptance, and to stand in need of no detailed criticism, and it will be sufficient to call attention to the names of the editors as a guarantee that its matter is thoroughly up to the level of our existing stock of knowledge. This abridgment is an accurate and well-condensed manual, admirably calculated for all medical and pharmaceutical students who desire a full acquaintance with the *Materia Medica*.

PART III.

HALF-YEARLY REPORTS.

HALF-YEARLY REPORT ON PUBLIC HEALTH.*

By CHARLES A. CAMERON, Ph.D., M.D., L.K. & Q.C.P.;
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on Chemistry in Steevens' Hospital Medical College, and the
Ledwich School of Medicine; Analyst to the City of Dublin.

SANITATION IN THE UNITED STATES.

UNTIL within a recent period very little attention was bestowed upon the subject of public hygiene by the governing bodies in the United States of North America. Notwithstanding the great prosperity and the rapidly increasing wealth of those States, only the most insignificant fractions of the public revenue were devoted to the purpose of improving the public health. This important subject is now, however, beginning to enlist the active sympathy of the State Legislatures; and before many years shall have elapsed there is little doubt but that public sanitation will be carefully and liberally attended to by the legislative and executive bodies throughout the Union.

In a former Report we described the Public Health Department which had been organized for the City of New York and its outlying districts. We now learn that this Department has been abolished, and that in its place another has been established for the City of New York alone. It is styled "The Board of Health of the Health Department of the City of New York." We presume that the Act of the Legislature of New York which dissolved the connexion in sanitary affairs between the City of New York and the outlying towns and rural districts, provided a sanitary administration for the latter. It looks, however, very like a retrograde

* The author of this report will be glad to receive any books, pamphlets, or papers relating to hygiene, dietetics, &c. They may be forwarded through the agencies of this Journal.

step to split up a large sanitary district into fragments. In these countries almost every sanitarian would be glad to see the innumerable sanitary authorities which exist in each county merged into one comprehensive and thoroughly organized Board of Public Health. Why should not the Corporation of London and all the *vestries* of the Metropolis constitute one great health authority, instead of, as at present, a score of Boards of Health? The Metropolitan Board of Works has, during the last ten years, done more for the improvement of the appearance, public convenience, and health of the Metropolis, than all the *vestries* would have accomplished during the next century. In the City of Dublin, with a population of a quarter of a million, there is a Public Health Committee, with a large staff; in the eight townships surrounding the City, and embracing a population (mostly rich) of nearly 70,000 souls, the public health is cared for by two or three "Inspectors of Nuisances." Would it not be desirable to form the City of Dublin and the eight townships adjacent to it, into a Metropolitan Health District? Union is strength in sanitary, as well as in military, matters; and if the small townships which individually are unable to pay for the maintenance of an adequate sanitary staff, would unite with each other and with the City, their combined resources would then sustain an efficient sanitary organization.

The new Board of Health of New York consists of the Police Commissioners, the "Health Officer of the Port," and four "Commissioners of Health" appointed by the Mayor, and holding office for a period of five years. Two of the Commissioners of Health must have been practising as physicians for a period of five years previous to their appointment.

The new Board was created on the 11th April, 1870, and a large volume^a just published by it details its operations during the first year of its existence. The staff of the Board consists of a "City Sanitary Inspector," and ten "Health Inspectors"—all Doctors of Medicine; a Sanitary permit Inspector, an Inspector of street cleaning (who is also a Doctor of Medicine), a Registrar and a Deputy Registrar of Records (both M.D.s too), a lawyer, an engineer, a chemist, an assistant chemist, a chief clerk, and a host of subordinate officials, including several medical men. The expenditure of the Board during the year amounted to nearly £34,000.

^a First Annual Report of the Board of Health of the Health Department of the City of New York, April 11, 1870, to April 11, 1871. New York: 1871.

The operations of the Board were chiefly as follows. The streets were subjected to an extra amount of cleansing, as compared with former years. House garbage and all kinds of offensive rubbish were removed by contractors of the Board; and the house refuse was, whenever deemed necessary, disinfected before being removed. Stable manure and dead animals were expeditiously conveyed from the City. The tenement houses were constantly and minutely inspected. The population of New York is 943,842, of which less than one-half occupies the tenement houses. In 1868, 78 per cent. of the deaths in New York occurred in the tenement houses; in 1869 the proportion fell to 68, and in 1870 it was still further reduced to about 66 per cent. This remarkable decrease—9·5 per cent.—in the mortality of the inmates of lodging houses, the Board attributes to the sanitary reformation which it and its immediate predecessor (the Metropolitan Board of Health) carried out in these dwellings. The work done in this department is, we are informed, recorded in “twenty-five bound volumes, which are constantly open to the examination of the inspectors, aiding them in their duties, and proving invaluable for reference.” We believe that the most important work of sanitary authorities is the thorough inspection of tenement houses, and the immediate rectification of any sanitary defects thereby revealed. This is the good sanitary work which in the City of New York is rewarded by the annual saving of 2,600 lives, and the prevention of a large amount of disease. The disinfection of the homes and clothes of those who had been affected by contagious disease, the compulsory removal of patients labouring under certain zymotic affections to hospital, the adoption of measures to rescue drowning persons, the chemical examination of petroleum, milk, and other articles, the supervision of dairy yards, abattoirs, &c., and the collection of vital statistics constitute, together with the duties already mentioned, the more important functions discharged by the New York Board of Health—an organization which we should indeed be glad to see imitated in every large town in the United Kingdom.

In New York the deaths in 1870 amounted to 27,175, being in the ratio of 28·79 per each 1,000 of the population—rather a high mortality. It is, however, maintained, and apparently with justice, that the registration of deaths is more perfectly carried out in New York than in any other City of the world. The City being built on an island—the avenues of egress to which are under strict surveillance—and the cemeteries being wholly under the control of the

municipal authorities, render it improbable that any interments could take place surreptitiously. In England and Wales it is estimated that no less than 50,000 deaths annually take place, which are not registered. In Ireland, as we have shown in a former Report, the vital statistics are collected so imperfectly as to be positively misleading.

More than 30 per cent. of the deaths in New York result from zymotic diseases. Small-pox, scarlatina, relapsing, typhus, typhoid, remittent, intermittent, and yellow fevers, measles, diphtheria, whooping cough, syphilis, and diarrhœal diseases are the chief zymotics. In New York, as in other cities, relapsing fever was almost invariably associated with poverty; it was found only in the most wretched purlieus, and amongst persons who were suffering from deficient nutrition.

The Board of Health of Massachusetts have published their Third Annual Report,^a a "Blue Book" of 329 pages. The sanitary authorities of Boston are not so liberally provided with funds out of the public treasury as the New York Board of Health. They do not appear to have a properly organized staff; and the sanitary work discharged by them during the year 1871 appears to have been chiefly in the abolition of nuisances arising from cattle slaughtering, melting fat, and bone boiling. We find, however, that the various towns of the Commonwealth of Massachusetts have their local public health authorities, who deal with the local nuisances; but we also infer from certain passages in the article on "the Health of Towns" in the report, that the local sanitary authorities are by no means efficient. One may easily imagine what a competent sanitary authority the City of Boston has when the secretary of the State Board of Health recommends that it should be remodelled so as to include two or more physicians, a lawyer, and a civil engineer. In the article on the health of towns we expected to find tables of the vital statistics of the state; but the only information of the kind given is the death-rate—23·5 per 1,000—of Boston during the year 1871.

Although the Report of the Massachusetts Board of Health does not contain anything like the record of sanitary reforms which we find in the New York Public Health Reports, still the volume includes a great number of interesting papers on various hygienic subjects; and though inferior in originality and importance to the

^a Third Annual Report of the State Board of Health of Massachusetts. January, 1872. Boston: Wright and Potter. 1872.

reports issued from the Medical Department of the Privy Council, it somewhat resembles in its subject matter Dr. Simon's valuable volumes. More than one reference to this Report will be found further on.

California is one of the states of the Great Republic in which we might least expect to find a public organization for the promotion of health and the prolongation of life. We learn, however, that there is a State Board of Health, the executive officer of which is Dr. Thomas A. Logan, President of the American Medical Association, and a most distinguished sanitarian. Its first (a biennial) report^a for the years 1870 and 1871 has been published by this Board of Health. It contains elaborate tables, showing the statistics of births, deaths, and marriages, and co-ordinate facts; and it includes a large number of articles on sanitary subjects. The death-rate in California averages 18·8, or one death in every 53 persons living. This mortality is low. In England the death-rate is 22 per 1,000 living. If the vital statistics of California are to be relied upon it must be one of the most healthy countries in the world. The birth-rate is very low—only 1 in 131·4 persons. The female population in California is relatively deficient in numbers; and the registration of births is stated to be defective: still the small number of births recorded is very remarkable. The Board of Health attributes the low birth-rate to some extent to the general and unnatural aversion of American mothers to the rearing of children, and to the ignorant, conscienceless abortionists and midwives who are permitted to ply their avocation. In most of the towns in the United States the birth-rate is very low; it is only 1 to every 87 of the population of New York. Dr. Logan shows by meteorological and other statistics that California is well adapted for phthisical patients. The mortality from pulmonary diseases is 16 per cent. of the deaths from all causes; but no inconsiderable proportion of this mortality is amongst phthisical patients who make California a health resort.

Apropos of Dr. Logan's phthisis statistics, we find in a recent number of the *Gazette des Hopitaux* a report of a lecture upon the mortality from this disease, delivered by M. Constantine Paul. The lecturer stated that the deaths from phthisis per 100 deaths from all causes were 6 at Rome, 8 at Naples and Venice, 9 at Turin, 9·7 at Genoa, 12 in England, 13·4 at Paris, 16 in Belgium,

^a First Biennial Report of the State Board of Health of California for the years 1870 and 1871. Sacramento: D. W. Gelwicks. 1871.

17·5 in Berlin, 20 at Vienna, 21·7 at Hamburg, 25·6 at Frankfort, and 28 in some parts of America. If these statistics be correct England is not after all the "home of consumption," as so many writers have stated it to be.

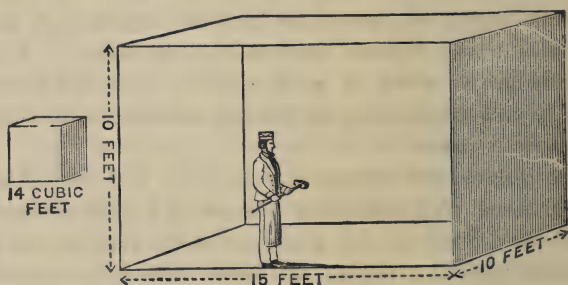
HYGIENE IN SCHOOLS.

No inconsiderable proportion of the life of the inhabitants of civilized countries is expended in schools and colleges; the hygienic condition of these places, and the physical culture of the bodies of those who spend their time in them are matters of great importance to the community. A large number of educational institutions exist on very unhealthy sites; and in too many of them there is a serious deficiency in the arrangements for ventilation. They are also frequently supplied with water of bad quality; and, in numerous instances their sewerage system is out of order. We have lately inspected the sanitary condition of several public schools, and in each we found the most serious hygienic defects. The case of Mercer's endowed school for girls at Ashtown, in the County of Dublin, will serve as an illustration. In this institution, which is situated in the open country, the girls are extremely well fed and comfortably clothed; they are provided with ample playground, and they sleep in well-ventilated dormitories. Notwithstanding these advantages their health is not nearly so good as that of the inmates of another school under the same management, who are not supplied with equally good food, and who live in one of the most densely populated parts of the City of Dublin. On examining the state of Mercer's School we found quite sufficient to account for the delicacy of some of its inmates. The main sewer was choked up; and an untrapped opening in the bath allowed the sewage gases to enter in to the bath room, and from thence to diffuse themselves throughout the house. The well water, too, was impregnated with sewage, which escaped from the blocked-up sewer. It contained 56·56 grains of solid matter per imperial gallon (70,000 grains weight), of which 22·6 grains consisted of organic and volatile matters. There were large quantities of free ammonia, albuminoid nitrogen, nitrous acid, and nitric acid in it. In this case the school authorities (one of whom is the Most Rev. Dr. Trench, Archbishop of Dublin, and who for nearly three hours was present at one of our examinations) did their duty in making a thorough investigation into the cause of the unhealthiness of the institution; but we fear that in but too many instances those in

whom the management of scholastic and eleemosynary institutions is vested never think of ascertaining whether or not their sanitary condition is satisfactory.

The recent removal of some of the large endowed schools from towns to suburban or rural districts is literally a move in the right direction, which we hope will ere long become general. Could there be a more unhealthy site for example than that of the Dublin Marine School, situated as it is on the banks of a river into which the sewage of a city of a quarter of a million inhabitants is discharged, and supplied with air contaminated with the exhalations of chemical, ammonia, coke, tar, gas, and artificial manure works? Children are less able to resist the effects of excess of cold, or heat, and of deficiency of food than adults; they also are more seriously affected by an impure atmosphere. A young man may endure without permanent injury a few years exposure to insanitary influences; but if the period between infancy and adolescence be passed under conditions unfavourable to the vigorous development of the body, then indeed it is rarely that the child attains to a strong and healthy manhood.

The malign influence which schools exercise on the health of children attracted attention many years ago. Carmichael, in his work on scrofula, published in 1810, proved that the excessive amount of phthisis which he found in many of the Dublin schools was caused by defective ventilation, and not by insufficient nutrition.



In the children's department of the House of Industry, the cases of scrofula were so numerous that the disease was believed to be present in a contagious form. In one of the wards, 60 feet long by 18 feet broad, there were no fewer than 38 beds, each containing four children! The amount of square feet allowed to each child was, therefore, only $6\frac{3}{4}$. The height of the ward is not given in

Carmichael's work, but assuming it to have been 15 feet, that would have given to each child 102 cubic feet.*

Neil Arnott and other authorities have placed on record cases of defective ventilation in schools almost as bad as those discovered by Carmichael; and if a careful inquiry were instituted at the present time, we have no doubt but that, even in the matter of ventilation, a large proportion of our schools would be found in a bad condition. The amount of carbonic acid gas in pure air is 0·04 per cent. When this proportion is doubled the state of the atmosphere is unsatisfactory. Roscoe found in the air of a school-room containing 22,141 cubic feet, and in which 164 boys were studying for $2\frac{1}{2}$ hours, 0·2371 per cent. of carbonic acid; and in another school-room he found 0·31 per cent. of this gas. Da Luna found in a room of a girls' school, at Madrid, the enormous amount of 0·723 per cent. of carbonic acid, or eighteen times the normal proportion of that gas.

Physical exercise is now a feature of school and college life, and more especially so in these countries. A friendly emulation amongst the scholars of the same educational institution, and between rival schools and universities in the performance of rowing, cricket, and other robust games, does much for the improvement of the physique of the rising generation. And it is in favour of these competitions that they have not a brutalizing tendency, like some of the pastimes of our forefathers. The drawback to the system of competition in athletic exercises is the undue strain which it so often imposes upon the muscular system, and which sometimes occasions permanent injuries, and even fatal lesions. Cricket is, perhaps, the game which is most unlikely to do violence to the important muscles; whilst it has the advantage of being, at least, a semi-intellectual game.

With respect to the ordinary gymnastics in boys' schools, they are, on the whole, well contrived to produce a healthy development of the muscles. In France this kind of physical education has been brought to a high degree of perfection—thanks to the teachings and efforts of A. Thierry, Berard, Colonel Amoros, Beclard, Bouvier, Londe, and Milne-Edwards.

That veteran sanitarian, Mr. Edwin Chadwick, C.B., has, in a

* In Dublin the minimum cubic space per head in the registered lodging-houses is 300 feet. The engraving (page 400) shows the relative sizes of a man, of the quantity of air which hourly passes out of his lungs, and of an apartment in which there is the minimum sleeping space for five persons.

recent communication to the *Journal of the Society of Arts*, pointed out the more common defects of the ordinary schools of these countries. He maintains that the chief sanitary defects of our schools are—1. Defective ventilation; 2. Defective warming; 3. Bad drainage and foul latrines; 4. Want of means of maintaining personal cleanliness; 5. Bad lighting; 6. Bad arrangements of desks and seats; 7. Want of proper means of gymnastic exercises; 8. Insufficient and ill-paved play-grounds. He submits that it is important that school boards should require, in the competition for plans, that these evils should be first considered and provided for, and that the architectural designs and elevations be made of secondary consideration.

It is painful, he says, to observe the condition of children in the common schools in winter time, going there in cold and wet, in driving sleet and snow, frequently ill-shod, and commonly ill-clothed—kept in the school during excessively long hours under any conditions for children, with feet and hands painfully cold—fingers often so benumbed as to be scarcely able to hold their slates and pencils; the open fires at one end of the school not freely to be approached, and, when approached, the warming or heating on one side, “roasting in front and freezing behind,” so as to give inflammations or colds, from the disturbed and unequal circulation. The confinement of children for five or six hours under such conditions, overtasked mentally, and painfully constrained bodily, surely requires active intervention for their relief.

Mr. Chadwick considers that of the modes of warming, those by hot-water pipes and iron surfaces are of inferior, and sometimes, when for high heats, of pernicious, effect; besides they are very expensive. They are apt to warm only the sides of the rooms, or the upper parts of them, and to leave the feet cold, unless an inconvenient and objectionable degree of heat is created over the whole room. It is, moreover, matter of considerable experience that warming by earthenware substances, or stone substances, especially by heat diffused over wide earthenware, or concrete surfaces, is more agreeable and more salubrious than any warming by iron surfaces.

In Germany, attention has been called to the poisoning of the air of school-rooms by carbonic acid passing into it through the sides of iron stoves. Dr. Oidtman, in a pamphlet on this subject, published in 1868, states that chronic poisoning of children by carbonic oxide is very common. We agree then with Mr. Chadwick in considering the principle of floor-warming, which he has so long

advocated, as the best. In some of the public schools, too, such a plan would prevent the tyranny of the larger boys excluding, as we know they do, the weaker and more delicate children from snug places beside the stove or fire-place.

In the large institutions, where children are boarded, the effects of progressive sanitary improvement have been distinctly marked. In one, where the death-rate had been about twelve per thousand, the foul air from cesspools and bad drains was excluded, the latrines were mended, and the ventilation was improved, when the death-rate was reduced to eight in a thousand. Next, regular tepid ablution, and, in summer time, cold water bathing, and careful skin-cleanliness was introduced, and the death-rate was reduced to four in a thousand.

Mr. Chadwick advocates the washing of children at schools, for various reasons, hygienic and otherwise. There is, in well-appointed schools in Holland, usually a female attendant on the schoolmistress, who takes the dirtied children into an apartment and washes them, the schoolmistress herself being above such a service.

The celebrated Rudolph Virchow, Professor of Medicine in the University of Berlin, has written (in 1869) a valuable treatise^a on the diseases incidental to schools, which we should be glad to see translated into English, and circulated amongst the school authorities of these countries. Virchow agrees with those orthopedists who maintain that the school is largely to blame for distortions of the spine, and more especially for that form of spinal curvature termed *Scoliosis*. He quotes several eminent authorities, amongst others Guillaume, who found amongst 731 scholars whom he examined, no fewer than 218 with distortion. The great majority of cases of scoliosis are amongst girls. In 72 cases noticed by Knorr, of Munich, there were 60 females. As girls spend less time at school than boys, and fewer girls attend at school, it has been urged that scoliosis is not most frequently induced by bad postures whilst studying. To this objection it may be answered that boys during their hours of play counteract by vigorous exercises involving the play of nearly all the muscles of the body, the evil influence of the school-room postures. On the other hand, girls, as a rule, do not practice any kind of gymnastics.

In almost every school the children in each class, no matter their heights, have to sit at desks of the same size; why could not

* Ueber gewisse die gesundheit benachtheiligende einflüsse der schulen.

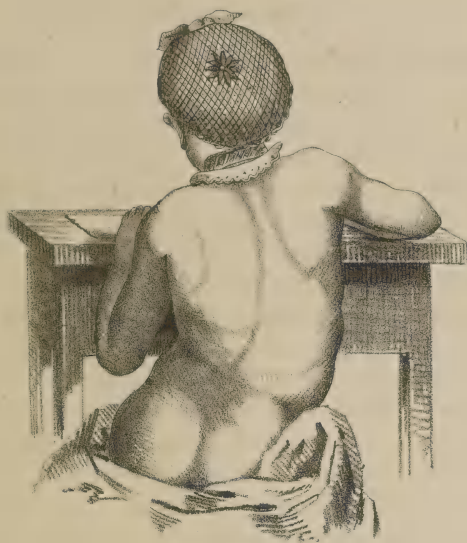
the desks for each class be made in short lengths, and of different heights so that no child would be placed at one either too low or too high for him or her? The bad position caused by writing at too high a table is shown in the engravings.

Virchow attributes a large proportion of the pulmonary consumption of childhood to over-crowding in school-rooms, to sudden changes of temperature in passing from hot school-rooms into the cold outside air, to the dust of the school-room, and, lastly, to impaired respiratory movement induced by prolonged sitting.

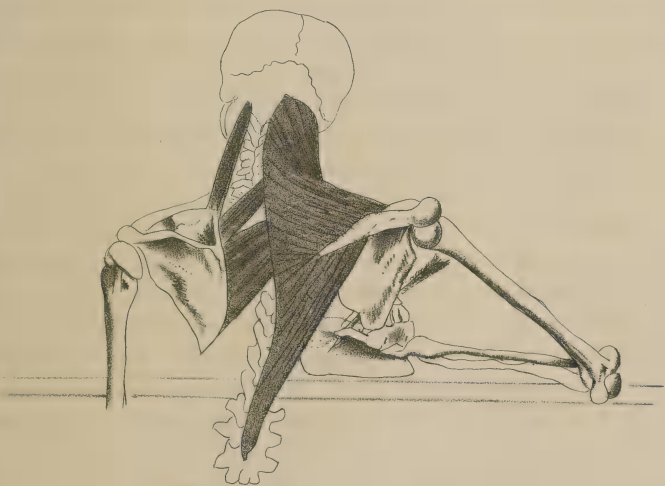
Short sight is the commonest disease in Germany. Dr. Cohn found that 60 per cent. of the students of the University of Breslau were near sighted. He blames the school-room for a large proportion of this disease. The rooms are not properly lighted, which obliges the scholars to stoop over their books. This causes a strain upon the muscles of the eye, producing an increase of hydrostatic pressure on the posterior portion of the eye-ball, and a lengthening of the axis of the eye. This state of tension, if prolonged, produces a permanent elongation of the axis of the eye. Besides, the stooping position determines an increased flow of blood to the eye-ball, and thereby augments the pressure on the back part of the eye. Virchow gives a list of various other diseases which originate in, or are aggravated by, the insanitary condition of school-rooms.

The *Lancet* for March 30th, 1872, calls attention to the deplorable condition of the children in pauper schools. At Mitcham, Islington, Anerly, and other schools of the same class, there are from 10 to 15 per cent. of the children suffering from ophthalmia. The government inspectors attribute the ophthalmia to constitutional defects, but the *Lancet* very properly remarks, why is it that the very same classes of children "running about naked and shoeless in the streets escape?" The cause of the ophthalmia is unquestionably due to overcrowding and impure air.

All our schools should be as regularly inspected by public officers as workshops and factories are. There are many laws on our statute books which relate to the health of men, women, and children employed in mines, factories, and other places; why should there not be equal provision for ensuring the health of the millions of children at school in these countries? Amongst Her Majesty's Inspectors of Schools there ought to be a few Medical Inspectors. Let us not forget the old adage, *mens sana in corpore sano*.



BAD POSITION CAUSED BY SITTING AT TOO HIGH A TABLE



BAD POSITION WHEN WRITING

THE ALCOHOL QUESTION.

An important joint declaration respecting the medical use of alcohol has been published by a large number of the leading medical men of London, and by some provincial and Irish and Scotch practitioners. The declaration is signed by the Presidents of the Colleges of Physicians and Surgeons, London, by the medical attendants of the Royal family, and by a large number of the most distinguished physicians and surgeons in England. The following is the document:—

“As it is believed that the inconsiderate prescription of large quantities of alcoholic liquids by medical men for their patients has given rise, in many instances, to the formation of intemperate habits, the undersigned, while unable to abandon the use of alcohol in the treatment of certain cases of disease, are yet of opinion that no medical practitioner should prescribe it without a sense of grave responsibility. They believe that alcohol, in whatever form, should be prescribed with as much care as any powerful drug, and that the directions for its use should be so framed as not to be interpreted as a sanction for excess, or necessarily for the continuance of its use when the occasion is past.

“They are also of opinion that many people immensely exaggerate the value of alcohol as an article of diet; and since no class of men see so much of its ill effects, and possess such power to restrain its abuse, as members of their own profession, they hold that every medical practitioner is bound to exert his utmost influence to inculcate habits of great moderation in the use of alcoholic liquids.

“Being also firmly convinced that the great amount of drinking of alcoholic liquors among the working classes of this country is one of the greatest evils of the day, destroying—more than anything else—the health, happiness, and welfare of those classes, and neutralizing, to a large extent, the great industrial prosperity which Providence has placed within the reach of this nation, the undersigned would gladly support any wise legislation which would tend to restrict, within proper limits, the use of alcoholic beverages, and gradually introduce habits of temperance.”

Dr. Ainstie, the editor of the *Practitioner*, and one of the most accurate investigators relative to the use of alcohol in health and disease, was requested to sign the above declaration, but he refused to do so, because whilst approving of the concluding paragraph, he entirely dissented from the views given in the first and second. He says:—

“It is, in the first place, both a false and a mischievous idea that any

considerable percentage of the drinking habits of any class of English society springs from the improper prescription of alcohol by medical men. That such improper prescriptions *may*, and occasionally do, cause drunken habits, I have not merely admitted, but elaborately proved on more than one occasion. But I protest now, as I have protested before, that in nine-tenths of the cases in which doctors have been blamed as the occasion of the evil habit, the accusation has been the mere invention of a lying drunkard anxious for a scapegoat to bear a portion of his disgrace. But the first paragraph of the declaration will undoubtedly be read by the public as a distinct admission that the medical profession has had a relatively considerable share in the spread of intemperance. I believe such a statement to be ridiculously false; and I would further suggest that, were it true, the eminent physicians signing this document, among whom are included a large majority of the teachers who have educated the recent generations of medical practitioners, ought to cast dust upon their own heads for having reared their pupils in such scandalous and mischievous ignorance of their duties.

“In regard to the second paragraph, I insist that it will convey to the public an entirely false impression as to the state of competent scientific opinion. It will undoubtedly be taken as a confirmation of the now obsolete doctrine of Lallemand, that alcohol is treated by the organism as a merely foreign stimulant, and is not decomposed therein. Now, I appeal fearlessly to Dr. Parkes (the only one of the signers of the declaration who has done recent experimental work regarding alcohol) to say whether his latest experiments have not strongly confirmed the refutation by Schulinus, Dupré, and myself, of Lallemand's doctrines regarding elimination of unchanged alcohol. In a letter which Dr. Parkes was kind enough to send me some few months since, he confessed, with that candour and high-mindedness which have ever distinguished him, that such was the case. I therefore believe that Dr. Parkes must have neglected to consider carefully the sense in which the declaration will inevitably be read by ignorant persons.”

Messrs. Gilbey, the eminent wine merchants, were the first to popularize—if we may use the term—the use of cheap wines amongst all classes of society. They are naturally interested in the alcohol question; and, therefore, they have published a statement which, to some extent, is intended to counteract the impression which the late medical declaration on alcohol is likely to make upon the minds of a large section of the public, and of Messrs. Gilbey's customers. They say that the way to combat intemperance is to provide the public with a cheap and abundant supply of wholesome liquor—discretion would then be the guide in

alcohol drinking. Anything that is cheap and easy to procure is not eagerly sought for. It is, however, a well known fact that in those colonies, such as, for example, Demerara, where alcohol is very cheap, drunkenness is an extremely prevalent, indeed almost a universal, vice; but the colonists have not probably lost the habits of their spirits-loving ancestors. The remedy for intemperance must be sought for in other directions as well as in that proposed by Messrs. Gilbey. Probably better education, and a cultivation of refined pleasures, will yet do much towards lessening the amount of intemperance which at present is a national reproach to the British people.

The Third Annual Report of the Board of Health of Massachusetts contains a mass of correspondence and statistics relative to the "use and abuse of intoxicating drinks throughout the globe." The information given is very interesting. It would appear that intemperance is, to a great extent, governed by a cosmic law. At the Equator there is very little intemperance, whilst the quantity and strength of the alcoholic beverages consumed increase steadily as we approach the North or the South. Between the isothermal lines of 77° Fahrenheit and 50° , there is some intemperance, but not of an excessive or a brutalizing nature. Wine, mostly "light," is the common alcoholic beverage used; and under its influence a man will *occasionally* "reel, staggering home, supported by wife and friend on either side. He is mild, and only unduly elated. His excitement bursts out into flashes of drunken wit or joviality. Rarely or ever does he become malicious or cruel. He is 'jolly drunk,' not 'crazy drunk.'" When we pass northwards of the isothermal line 50° Fahrenheit, we find brandy, gin, whiskey, and other strong distilled spirits taking the place of wine. Russia, the Scandinavian States, Holland, a small portion of Germany, and nearly the whole area of the British Isles lie north of the isothermal line 50° of mean annual temperature. The people here "drink deeply, and of more fiery liquor than the men of the south. Instead of simple exhilaration, such as is generally seen on the shores of the Adriatic and the vine-clad hills of Southern Germany and Spain, the dwellers along the Baltic and the Northern Seas drink even to narcotism, and lie in beastly intoxication, perchance in the very gutters of many a northern city."

As the United States lie equatorially from isothermal line 50° , we might naturally expect to find the people as temperate as the Southern Germans are; such however is not the case, for

intoxication is a widespread and enormous vice in every part of the Republic. The writers in the Report of the Massachusetts Board of Health say that these habits of intemperance have been imported from the British Islands by the early immigrants, and that they are maintained simply because the vine was not cultivated in the country. If vine culture had been introduced into the States two centuries ago, the native Americans would probably now be a temperate people, as they are constitutionally disposed to be in obedience to the cosmic law already referred to.

That the European immigrants retain the customs of their fatherlands is rendered painfully evident by the Police Statistics of Boston. In that city there are 56,900 Irish, of whom 14,673, or 25·78 per cent. were sent to prison in one year. The Germans number 5,606, and of these 364, or 6·49 per cent. were imprisoned in one year. Of course drunkenness was the prime cause of the offences committed by both the Irish and the Germans. The native Americans are not superior in point of temperance to any of the European immigrants: but they have the tastes for intoxication "which climatic law and long years of habits of intemperance on the part of our English ancestors have engendered;" and these influences are increased by the stimulating nature of the climate. The chairman of the Board of Health, and who is also the editor of the correspondence relative to intemperance, believes that the evil will always exist; but he contends that it might be greatly mitigated by encouraging the use of light wine and mild ales. On the other hand he (Dr. Bowditch) claims for alcohol high medicinal attributes, believing that physicians "do at times save human life by using various stimulating drinks with the utmost freedom."

ADULTERATION AND THE ADULTERATION LAWS.

Mr. Muntz, the Member for Birmingham, has introduced into the House of Commons a Bill for the Prevention of the Adulteration of Food, Drink, and Drugs. It differs very little from the present defective law relative to adulteration. At present a person convicted for selling as pure an adulterated or impure article of food or drink may be fined £5 and charged with costs; and on a second conviction the magistrate may order his name, address, and offence to be advertised in a newspaper, or in some other public manner. In Mr. Muntz's Bill it is provided that a person convicted of adulterating any article of food, drink, or drugs, may be fined £50, and on a second conviction for a similar offence be imprisoned

for six months with hard labour. A person convicted for merely selling an adulterated article may be fined £20, and on a second conviction his name, address, and offence may be advertised at his own expense. The Public Health authorities are empowered to appoint persons to collect samples of food for analysis, and to prosecute the vendors of the articles proved to be impure; nor need the persons appointed for this purpose notify to the vendor that they intend to have the articles analysed, as *all* purchasers are now obliged to do, if they intend to prosecute the vendors in the event of their purchases proving to be adulterated. These are the only changes which Mr. Muntz proposes to make in the existing law.

Since the year 1862, we have had considerable experience in the working of the present Food Adulteration Act, which, notwithstanding its many defects, has certainly proved of great utility in Dublin. The Corporation of Dublin are very anxious to fully carry out every measure relative to the suppression of the traffic in unwholesome and adulterated food, and their efforts, in relation to this object, compare favourably with those of the other municipal authorities in the United Kingdom.^a

The laws relating to the traffic in diseased and unsound food being very clear, and very stringent, the Dublin civic authorities have been able to almost completely prevent the disposal of the flesh of diseased animals, and of food in a putrid state, in this city. With respect to the sale of adulterated food, their efforts have not been so successful, because of the defective condition of the present law relating to that species of fraud. The defects which appear to be most glaring are as follow:—

1st.—The inadequacy of the punishment when convictions are obtained.

2nd.—That the officers of the civic authorities have not the power to seize upon articles which they have reason to suspect are adulterated, so that they may be submitted to analysis. At present the medical officer of health, or the inspector of nuisances, is empowered to seize upon any kind of food which appears to him to be unsound or diseased, and unfit for the food of man. Why, therefore, should not the officers of civic bodies, appointed for the purpose, have the power to take specimens of food exposed for sale,

^a More than 100 persons have been convicted in Dublin for selling adulterated food, and several fraudulent dealers have had their names, addresses, and offences publicly advertised.

and have them examined by the public analyst? In Dublin the sanitary officers of the Corporation are so well known to the dairy owners, that when they purchase milk for analysis, it proves as a rule to be pure, though the vendor may be notorious for the poor quality of his milk. Out of thirty-two samples of milk collected in one month for analysis, no fewer than eight proved to be cream! We do not suggest that the municipal officers should have the power to seize articles of food on sale in the markets and shops, but we do insist that they should be empowered to *purchase compulsorily* those articles that to them appear likely to be adulterated. In the present state of the law, the vendors of food may and do refuse to sell food to the sanitary officers. They should be punished for *refusing* to sell to the officers appointed for inspecting food.

3rd.—According to the present condition of the law, the purchaser of an article of food, who wishes to get it analysed, must at the *actual time* of the purchase give notice of his intention to the vendor. The object of this procedure is to allow the vendor to accompany the buyer to the public analyst, in order to see that the article was not tampered with *in transitu*. Now, when a person purchases an article of food, surely it is not ordinarily with the desire of getting it analysed, but with the intention of eating or drinking it? It is, therefore, only when the food is being about to be consumed, that the suspicion of fraud comes into the purchaser's mind. Of course, it is then too late to take the article to the analyst.

4th.—There is a looseness in the definition of the word “adulteration” as applied to food. For example, can skimmed milk be considered an adulterated article, seeing that nothing is put into it? Tea-leaves from which the soluble ingredients are extracted are sometimes re-dried and sold as genuine tea. In this case no foreign matter is mixed with the article. At a late milk adulteration case heard in Dublin, it was contended by counsel that milk deprived of a portion of its cream was not an adulterated article of food, whatever else it might be—an argument that appeared almost to convince the magistrate who heard the case. In the new measure for the amendment of the Food Adulteration Act of 1860, a careful definition of adulteration should be given. An adulterated article should be defined as follows:—Any article of food, or drink, or drugs, to which any unessential, foreign, or other substance is added, which has the effect of lowering the value of such article; and any article of food, or drink, or drugs to which is added any

ingredient or material calculated to be injurious to the health of persons who might use the same; and any article of food, or drink, or drugs from which a portion of its essential ingredients is or has been abstracted, shall be deemed to be articles which are adulterated, and not pure.

Mr. Muntz's Bill should be also amended, so as to enable counties and small county towns to appoint as their "Public Analysts" chemists residing in the larger cities. It is not likely that an analytical chemist would locate himself in such a town as Ballinasloe or Westport, or even in such a district as the county of Donegal, or the county of Clare. Samples of food might be purchased in the smaller towns, and sent to a chemist in London, Dublin, Glasgow, or other large towns; and his certificate should be received as evidence in court. Even under the existing law, the certificate of the public analyst is sufficient evidence without his sworn testimony. We have submitted to Mr. Muntz several amendments which, if adopted, would render his Bill a really practicable piece of legislation.

The Pharmacy Act is now incorporated with the Adulteration of Food Act; and consequently the sophistication of drugs is a punishable offence. We believe that adulteration of drugs is practised to a far greater extent than the sophistication of food. It is the poor who most suffer by reason of the fraudulently made-up drugs from which their medicines are dispensed in hospitals, dispensaries, and asylums. In the United Kingdom the exposures of the falsifications of medicinal substances, which have from time to time been published, are almost innumerable. Now a complaint of drug adulteration reaches us from beyond the Atlantic. At the recent Pharmaceutical Congress, held at St. Louis, United States, Mr. J. H. Remmington, of Philadelphia, read a paper on the adulteration of drugs. He showed that this practice prevailed extensively throughout North America. In many of the large wholesale houses special departments under particular managers are wholly devoted to the preparation of spurious and sophisticated medicaments, spices, chemicals, &c. Flour, starch, woody fibre, sawdust, sulphate of barium, and white clay are mixed in enormous quantities with more costly substances, and the compounds palmed off upon the public, and on the retail traders too, as the genuine articles. According to Mr. Remmington, spices are the articles most frequently and largely adulterated—an observation which applies with equal exactness to the spices vended in the United Kingdom.

It may be said of drugs, as of other articles of commerce, that whilst there is no absolute certainty of procuring them in a state of purity, yet that if a fair price be paid for them, and if they be purchased from respectable persons or firms, who have got good reputations in their business, there is every chance of obtaining them pure. The abominable contract system, under which the drugs supplied to most of the charitable institutions of the country are those offered at the lowest price, should be abolished. Respectable firms do not like to bind themselves to supply medicines at a fixed rate during a year, because the prices of those articles are constantly fluctuating. It would be far better if poor-law guardians and hospital authorities purchased good medicines just as they were required, paying the well-known current prices for them, and dealing only with the most respectable houses.

INFLUENCE OF MARRIAGE ON HEALTH.

In the elaborate treatises on hygiene, which have been written by such distinguished men as Levy, Motard, Londe, Tracy, and Broussais, the influence of marriage on health is treated more or less fully. These writers agree in attributing to matrimony a beneficial effect on life and health.

At a meeting of the French Academy of Medicine, held on the 14th of November, 1871, M. Bertillon read an elaborate paper on the influence of marriage on the duration of life, and on intellectual or moral diseases. His statistics chiefly apply to France, Belgium, and Holland. From 25 to 30 years of age, the married men die at the rate of 6; the unmarried 10; and the widowers 22 per 1,000 per annum. From 30 to 35 years, the deaths amongst these classes are respectively 7, 11, and $19\frac{1}{2}$ per 1,000; and from 35 to 40 years, $7\frac{1}{2}$, 13, and $17\frac{1}{2}$ per 1,000. At greater ages the same favourable difference exists in the case of the Benedicts *versus* the Celibates. It is curious that widowers are more likely to die than men of the same age, who have never been married. The exceptions to the low mortality amongst Benedicts are only in the case of those who marry very early in life. It is rather startling to youthful worshippers at the shrine of Hymen to be informed that married *men* from 18 to 20 die as fast as men aged from 65 to 70. Amongst women marriage is not quite so favourable to longevity. No effect is observed until after the age of 25 years. Spinsters from 30 to 35 die at the annual rate of 11 per 1,000; wives in the ratio of 9 per 1,000. The mortality is greater in the case of wives under 25

years than of spinsters below that age. After 40 years the longevity of married women is much greater than that of unmarried females of corresponding ages. Middle-aged widows do not live so long as middle-aged spinsters or wives. M. Bertillon shows that, according to the doctrine of chances, or rather probabilities, a man who marries at 25 years is likely to live 40 years longer, whilst his chance of living so long, if he does not marry, is reduced by five years. On the other hand, a woman who marries at 25 years is likely to attain the age of 65, whilst if she remain single she will only attain the age of 56 years.

According to Bertillon, crime is most rife amongst the unmarried, and least amongst the married. The widowers and widows are not nearly so bad as those who are unwedded, but they are not (of course we are speaking of averages) quite so virtuous as those who are actually in the holy estate of wedlock. On the whole, M. Bertillon's statistics are most cheering to the intending Benedicts.

In the manufacturing districts of England the bad influence of early marriages is rendered painfully evident by the wretched stunted children who are to be met with in every direction; for there children are the mothers of children, and couples are to be met with whose united ages do not exceed thirty years.

In the third Morisonian lecture, delivered in March last, by Dr. Austen Mitchell, before the Edinburgh College of Physicians, the subject of marriages between persons of consanguinity was treated at great length. The lecturer disapproved of marriages between near relatives. He did not, however, believe that the evil resulting from such unions arose from any mysterious influence intrinsic in the consanguinity itself. The danger, in his opinion, was due to the increased risks in such marriages of the transmission of abnormal peculiarities. For example, if a deaf-mute is married to a person endowed with the attributes of hearing and speaking, the chances of their having a deaf-mute child will be as 1 is to 135; but if deaf-mutes intermarry the chances that their offspring will be equally defective will be as 1 is to 20.

INFLUENCE OF THE USE OF SEWING MACHINES ON HEALTH.

During the last dozen of years many articles and reports on the effects of the sewing machine (worked by foot power) on the health, have been published. Dr. Vernois, in the *Annales d'Hygiène Publique*, Vol. VIII., 1862, ascribed to the use of these

machines irritation of the sheaths of the flexor and extensor muscles, inducing severe attacks of cramps, occasionally followed by partial paralysis. He further maintained that females whilst learning to work with these machines often laboured under a peculiar nervous excitement. On the other hand, Dr. William Ord, in his report on the sanitary conditions of the needlewomen of London, states that on the whole the sewing machine proves beneficial rather than injurious to them. It enables them to add 50 per cent. to their earnings; whilst the exercise of the muscles of the legs and trunk renders less injurious the effects of a sedentary occupation. He admits, nevertheless, that the cramped position of the operator sometimes occasion thoracic pain, and produces indigestion; and also, that occasionally, delicate women become exhausted by the unaccustomed physical exercise, just in the same way that a clerk, if not robust, would become over-fatigued by working all day with a spade. Dr. Guibout, Physician to the Hôpital St. Louis, Paris, and Dr. Fournier, shortly after the publication of Dr. Ord's report, published papers which gave a very bad account of the use of the sewing machine in the case of factory operatives. Dr. Espagne stated in the *Montpelier Medical Journal*, May, 1869, that although it would be desirable to work the machines by artificial motive power, yet when used by foot power they do not produce any general malign influence upon the health of those operating with them. In 1870, Dr. G. Decaisne published a very exhaustive memoir on this subject in the 35th volume of the *Annales d'Hygiène Publique*, second series. He showed that sewing by means of machines exercised no more deleterious influence upon the health than arose from long-continued needlework of any kind. In the third annual report of the Massachusetts Board of Health there is an elaborate article on the hygiene of sewing machines, written by Dr. Arthur H. Nichols. The following query was addressed to a large number of persons:—"Have you observed any injury to health from the use of sewing machines moved by foot power? If so, please to send us all the information you may have on the subject." 138 replies were sent in from 120 towns in the State of Massachusetts. In 80 of the replies one or more instances of ill effects produced by the machine are recorded, and in 58 returns the answers are in the negative, or are doubtful. In the 58 returns, however, the information given relates chiefly to towns where the machines are only used in private families. Dr. Nichols, having carefully analysed the returns,

and made due allowance for exaggerations on both sides, evolves the following conclusions from them:—

“*First.*—That the sewing-machine may be used by a healthy woman of average strength for three or four hours daily (a length of time sufficient for the work of an ordinary family) without causing excessive fatigue, nor any appreciable ill effect.

“*Second.*—That the illnesses which most frequently prevail among professional operatives making use of the treadle are:—

“*a.* Indigestion, attributable to the unhealthy conditions in which they pursue their occupation, particularly the impure atmosphere of the work-rooms, the sedentary employment, and want of open-air exercise.

“*b.* Muscular pains, affecting the lower limbs and trunk, produced by the long-continued, frequent use of the same muscles.

“*c.* Diseases peculiar to women, aggravated by, rather than caused by, the plethoric condition of the pelvic organs, induced by this exercise.

“*d.* General debility. By this is meant a state of physical deterioration and nervous prostration brought on by overwork.

“*Third.*—That other ill effects, such as neuralgia of the feet, from contact with the iron treadle, affections of the spine, as well as the nervous effects described by M. Guibout, are worthy of mention only from their extremely rare occurrence.

“*Fourth.*—The unhealthy tendencies of this occupation may be greatly diminished by the substitution, if practicable, of some other motive-power than that of the feet, or the adoption of one of the improved treadles above described. It is, moreover, of very great importance that, in those establishments where large numbers are congregated, attention should be directed to the proper ventilation of the work-rooms.”

THE PUBLIC HEALTH IN 1871.

The recent severe illness of his Royal Highness the Prince of Wales has been the means of directing public attention to the sanitary condition of the towns and dwelling-houses of these countries. It has had the effect of literally making thousands of people put their houses into good order. Many local sanitary authorities have for the first time in their corporate existence given a thought to the state of the sewerage system within their respective jurisdictions. Greatly as we regret the sufferings which the Prince of Wales has lately endured, yet the illness of that illustrious personage cannot be regarded as an unmitigated evil, since it has served to call attention to those sanitary defects which annually occasion an enormous waste of human life in these countries, and which are to a great extent easily remediable.

Although very careful inquiries have been made with the object of ascertaining the source of the typhoid poison which infected the Prince of Wales, the matter has not been satisfactorily determined. The sanitary condition of Sandringham, the Prince's country residence, has been favourably reported upon, with the exception of the water. The water which supplied the Mews was exceedingly impure, and perhaps from this source the Prince's groom became infected with the poison of typhoid. At the village of Sandringham there have for some time past been more cases of enteric fever than we might reasonably expect to find in so small a community.

Londesborough Lodge, where the Prince actually sickened, is situated on the outskirts of Scarborough, and it is drained by the main sewer of the town. The *Lancet* and the *British Medical Journal* published, during the Prince's illness, special reports upon the sanitary condition of Londesborough Lodge; and both reports agreed in condemning the sewerage arrangements. Scarborough being a port, the contents of its sewers are discharged into the sea, and during a portion of the day the mouths of the sewers are sealed by the tide, and the sewer gases are forced back into the town and into the houses. The main drain at Londesborough Lodge runs right under the house, which is very objectionable. After the publication of the reports of the medical journals, Lord Londesborough had the sanitary condition of the Lodge re-investigated, when it was discovered that the sewers were provided with ventilating shafts, which carried any gases that might be generated in the sewers to points higher than the chimney-tops. The sanitary condition of Scarborough being called in question by the *Builder* and by several writers, the local authorities requested Mr. Bazell-ette, the well-known engineer, to investigate into the sewerage of the town. His report, dated the 18th March, is favourable. There are in the borough 21 miles of roadway built upon, whilst the sewerage extends to 22 miles. He suggested a few improvements which might be effected at a trifling cost. The Special Sanitary Committee of Scarborough have issued a report, dated 28th April, 1872, in which they state that their engineers have established the fact that for the last year the district in which Londesborough Lodge is situated, and which contains one-sixth of the inhabitants of the borough, has had a remarkably low death-rate. They further affirm that no death from a zymotic disease occurred in that district during the year. Under those circumstances, those

valetudinarians who intend to make Scarborough a health-resort, need be under no apprehension that they will incur unusual risk from zymotic diseases whilst sojourning in that pleasant watering-place.

During the year 1871 the death-rates per 1,000 persons living in the 20 principal towns of the United Kingdom were as follow:—
Portsmouth 19, Hull 23, Bristol 23, London 25, Birmingham 25, Norwich 26, Bradford 26, Nottingham 26, Dublin 26, Leeds 26, Edinburgh 27, Leicester 27, Wolverhampton 28, Sheffield 28, Salford 30, Manchester 31, Newcastle-on-Tyne 32, Glasgow 33, Liverpool 35, and Sunderland 37. The average mortality in the 20 towns was 27 per 1,000 persons living. In the 17 largest towns of England small-pox carried off nearly 16,000 persons during the year.

Mr. Robert Lawson, F.R.C.S.L., at a recent meeting of the Epidemiological Society, read an interesting paper on the spread of cholera through European Russia last year. The chief points of interest, with regard to this epidemic were brought before the Society, illustrated by charts, showing at a glance the march of the cholera, and the degree of intensity with which it prevailed in the several governments of European Russia. The whole of the official returns, so far as yet published, were also shown, arranged, and tabulated according to the various governments, giving averages of mortality per 1,000 of deaths to cases (or attacks), and of percentage of cases to population. The census returns quoted were those of 1870, just published at St. Petersburg, together with the meteorological and other tables. The comparative method should be always used by way of crucial test in all epidemiological study; one should never rely solely upon, singly, the pathological, physico-chemical, statistical, or meteorological aspects of the question.

The co-incident phenomena or favouring conditions observed at the local "spring" outbreak at St. Petersburg, with tables of disease, statistics arranged in daily and in weekly periods, showing its "epidemic vital force," were then reviewed in detail.

The summer heat diarrhœa should always be eliminated, being entirely distinct from epidemic Asiatic cholera. The relation and connexion between an epidemic of enteric fever already existing over a defined area, with that of an invading wave of cholera, with their correlation and influence on each other, as met with in other zymotic diseases; and the prevalence in 1871 and previous years of

epizootic and epiphytic diseases in Russia were then succinctly treated.

The other points brought forward were:—

The rapidity and almost day coincidence in point of time with which the cholera appeared at different places in so wide an area as European Russia. On this head remarkable facts were brought out clearly by chronological and geographical charts of epidemic cholera, not only in Russia, but also in India, Persia, Zanzibar, &c.

Its apparent localization, as at Kieff since 1868; thus waiting for the next and succeeding years before vitalizing into its epidemic and spreading form.

The condensation, over-crowding, and the insanitary condition of inhabitants either in town or country, always found to be a constant factor, and to exert a vast and overwhelming influence on the existence, spread, and death-rate of cholera.

Cholera has not this year lived or become vitalized in any of our ports. Important questions for the epidemiologist to consider are: Will it behave in like manner in 1872? Or will there be “spring cholera” in 1872 in Northern Europe? Is there a “wave of typhoid” in advance of it? Will this direct, influence, or modify in any degree the progress of cholera westward?

The *Medical Times and Gazette* gives the following *resumé* of a communication made in January, 1872, to the French Académie de Médecine, by M. Fauvel:—

“The cholera, then, the invading progress of which towards the North-east of Europe has for the time been suspended, still prevails with some intensity at Constantinople, menacing thence all the basin of the Mediterranean, which to the present time has remained intact. On the other side, the disease, advancing through Arabia to the holy places of Islamism, threatens the invasion of Egypt, and consequently the shores of the Mediterranean, as in 1865. This is the present position of Europe with regard to the cholera: and it follows that, if we have some chance of escaping a scourge which presses on us on several sides, there is also a strong probability that we shall be subjected to its invasion. This the year 1872 will decide.

“There is, however, some compensation in this perspective. If we cast a comprehensive glance on the invasion of the cholera in 1871, we perceive, without any doubt, that at no epoch has the disease prevailed over so vast a space; for we find it prevailing with varying intensity along an undulating line, which is scarcely interrupted, from Archangel to the southern extremity of Africa. But, then, in compensation, never has an epidemic of cholera shown itself so benign in Europe as that of

1871. It is not that the malignity of the disease has diminished, for the gravity of the attacks has continued the same; but, with certain exceptions, their number has been much more limited than usual. In other words, individual resistance opposed to the action of the morbid principle has been more extended than in former epidemics. The progression towards Western Europe has also been less active, and—a fact to be noted—it would seem that, wherever the means of disinfection have been employed with energy and intelligence, they have greatly contributed to the extinction of epidemic foci, and, consequently, to attenuate their effects. The conclusion to be drawn from this last fact is, that if the cholera should unfortunately visit our country, it ought to find us prepared beforehand to oppose it with those prophylactic measures which experience has proved to be of service.”

The Secretary of State for India has just issued a large Blue Book on the sanitary condition of that extensive region. It includes a large number of official reports from the authorities in the different presidencies; and it contains elaborate directions for the performance of water analysis, by Drs. Macnamara, Parkes, and Angus Smith. In this report the etiology of cholera is discussed at considerable length. Dr. Cunningham concludes from his observations and information that the spread of cholera in India is not altogether in harmony with either Pettenkofer's (the soil water) theory, or with Dr. Bryden's (air spreading) hypothesis. The water-carriage theory is not fully supported by the facts referred to in Dr. Cunningham's review. On one point, however, the writers in the report are fully agreed, and that is, that whatever may be the *materies morbi* of the cholera, and the *vera causa* of its epidemics, the spread and fatality of the disease are greatly influenced by the insanitary conditions of the district which it invades. “The degree in which inhabitants of a given area are likely to escape will depend greatly on their sanitary condition, on the purity of the water supply, the excellence of the drainage, and the completeness of all other such arrangements.”

SOME LESSONS TAUGHT BY THE PRESENT SMALL-POX EPIDEMIC.

In the year 1870 small-pox, which had long been declining, became almost extinct in Ireland, and we were beginning to flatter ourselves that this pest would never return to afflict the people of this country. The steady diminution of variola since the

introduction of compulsory vaccination and the cheerful conformance of the vast majority of the people with that beneficial law seemed to imply that the great Jennerian prophylaxis would for ever rid Ireland of small-pox. Dr. M. Burke, however, said very sagely, when examined before a recent Parliamentary committee on vaccination, that no safe conclusion could be arrived at relative to the protective influence of vaccination in Ireland until it was tested by the occurrence of an epidemic. The epidemic came last year, and in Dublin alone nearly ten thousand cases of small-pox (out of a population of 310,000 persons), have occurred during the last twelve months, and more than a thousand persons have fallen victims to the disease. At first the cases of small-pox were confined to the poorest and most crowded localities; but latterly it has invaded the most fashionable quarters of the city, and is attacking persons amongst the highest as well as the lowest ranks of society. We propose to give a few statistics relative to the present small-pox in Dublin and to the recent outbreaks of that malady in other places, and to indicate some of the lessons derivable from them.

The city of Dublin is divided into two poor-law unions, each of which includes a few suburban districts. Dr. Burnside, one of the physicians to the South Dublin Union Small-pox Hospital, informs us that from November the 15th, 1871 (at which time the epidemic was beginning to assume large proportions), until the 11th April, 742 cases of small-pox were admitted into that hospital. Of these, 152 died, nearly 17 per cent. of the cases of admission. About 80 per cent. of the total cases were "modified," and 20 per cent. confluent, at least on the face. Of purpuric cases, the per centage was between 5 and 6, and although in a few of these cases there was recovery, the great majority rapidly terminated fatally. Out of the 742 cases, 596 were vaccinated in some way, and of the latter 64 terminated fatally. "Of re-vaccinated cases," says Dr. Burnside, "I cannot say I saw the disease occur in one when I could positively state that re-vaccination had been thoroughly performed prior to infection." Dr. Burnside states that he has seen mild cases of small-pox amongst the unvaccinated, but they were exceptional; the mortality amongst the 146 unvaccinated cases amounted to 88 per cent.

Dr. Joseph E. Kenny, Visiting Physician to the Small-pox Hospital (Sheds), established by the North Dublin Union, has kindly furnished us with the following statistics:—

Total number admitted into North Union Small-pox Sheds from 28th October, 1871, to 20th April, 1872, 544.

Vaccinated,	-	-	-	-	442
Unvaccinated	{ certainly,	-	-	71	102
	{ probably,	-	-	31	
<hr/>					
Total,	-	-	-	-	544

Died 113, or 20·77 per cent.

Number of deaths in unvaccinated and doubtful classes, 70, or 68·62 per cent.

Number of deaths in vaccinated class, 43, or 9·72 per cent.

The total is thus divided as to sexes—

Males,	-	-	-	-	313
Females,	-	-	-	-	231
<hr/>					
Total.	-	-	-	-	544

Died, males, 76, or 24·2 per cent.

„ females, 37, or 16·08 per cent.

There was a greater number over than under 14 years of age. The proportion of purpuric cases was very large, and in all such cases, when well marked, the result was fatal.

With regard to those cases which are marked as doubtful as to vaccination, Dr. Kenny says:—"I mean by that to express that I could not discover any marks, and the history given by patients or friends was too uncertain to be reliable. The weight of evidence in those cases is, in my opinion, in favour of their not having been vaccinated. In four cases the attack of small-pox was the second experienced by the patients. In no case have I seen a third attack. The intervals in the above four cases varied from 12 to 65 years. In only two cases has re-vaccination been performed, or, what amounts to the same thing, primary vaccination done within a period of three or four years. In both these cases the disease was in its very mildest form. I have seen seven cases where re-vaccination was performed within ten days of the attack of small-pox, but after, in each case, full exposure for several days to not only the infection, but also contagion of small-pox. In two of these cases the disease was very severe, and in one it proved fatal. My experience of vaccination as a prophylactic against, or a modifier of, an impending attack of small-pox, does not coincide with that of Dr. Furley, as published by him some time ago. The cases I

quote above, and about eight others in which I vaccinated the patients myself, do not support his theory on the subject, viz., that vaccination, if done either in the incubating stage of an attack, or even when the papules have appeared, tends to cut short or otherwise favourably modify the type of the disease. I do not think, as far as my experience goes on the matter, that this most desirable result is produced by his operation. I had tried it last November, several months before he had made public his observations. The subject, however, is, I think, worthy of further investigation. I cannot too strongly express myself in favour of re-vaccination, which I believe should be at least a decennial operation."

Dr. William Moore, King's Professor of Medicine in the University of Dublin, informs me that he had 37 cases of small-pox under his care at Sir Patrick Dun's Hospital. Of these, one (a middle-aged woman) was re-vaccinated, 31 were vaccinated, 2 had previous attacks of small-pox and had also been vaccinated, and 3 were non-vaccinated. The re-vaccinated patient died from confluent small-pox; one of the patients (a woman aged 44), who had previously suffered from small-pox, recovered after a mild illness: the other case, that of a man aged 20, was severe, being semi-confluent and purpuric. Of the 3 unvaccinated cases, 1 (a child aged 5 years), died, another had a severe attack, and the third (a child aged 3 years), appears to have a mild attack, as it remained in hospital only 5 days. Of the 31 vaccinated cases, 1 (a woman aged 38), was confluent and purpuric, and terminated fatally; the remaining 29 were modified, and for the greater part mild.

We learn from Dr. Moore's cases, although they are not numerous, that small-pox is rendered less fatal by vaccination, for, including the re-vaccinated cases, only 2 deaths occurred out of 31; whilst out of 3 non-vaccinated cases, 1 died. It is remarkable that the only case of small-pox after re-vaccination admitted should have proved fatal; but it appears that this woman had been attending her husband, who was ill with small-pox, and had been re-vaccinated whilst exposed to variolous infection. When admitted she had vaccinia and small-pox at the same time, and it is more than probable that the virus of the latter disease had entered her blood before she had been vaccinated. It is also rather remarkable that amongst 37 cases of small-pox there should be 2 patients who had previously suffered from the disease. We need not therefore be surprised to find cases of small-pox occurring amongst those who have been thoroughly re-vaccinated, for surely an attack of variola

ought to afford greater protection from a second one than vaccination could reasonably be expected to confer.

Dr. Moore says in the statement which he has kindly given to us—"I may take this opportunity of mentioning that I believe in the antiseptic properties of the sulphides, and especially of sulphurous acid, which I have given in almost every case, both public and private, I have been called on to treat. In addition, where one case has occurred in a family I have given it to the other healthy members as a "prophylactic," which, coupled with other due quarantine precautions (I believe), has tended to prevent the spread of the disease."

Dr. Grimshaw, one of the physicians of Cork-street Fever Hospitals, has favoured us with the following statistics relative to the admission of small-pox patients into that institution. From the 1st April, 1871, until the 31st March, 1872, the admissions amounted to 425. Of these, the vaccinated patients were 334, of whom 35 died; and the non-vaccinated cases number 91, no fewer than 70 of which terminated fatally. No case of small-pox after undoubted re-vaccination was admitted. These statistics having been made out in the latter part of April may be regarded as a full account of the termination of the cases admitted during March.

Dr. Lyons, one of the physicians to the Hardwicke Fever Hospital, read a paper on the statistics of small-pox, at a meeting of the Medical Society of the King's and Queen's College of Physicians in Ireland, held on the 17th April, 1872. In this paper, which is published in full in the present number of this Journal, the protection afforded by vaccination is clearly shown.

Of 541 vaccinated patients admitted into the Hardwicke Hospital, only 61, or 11·44 per cent., died; in 3 re-vaccinated cases there was no death; and of 66 non-vaccinated, and 4 doubtfully vaccinated, cases, 53, or 76·19 per cent., terminated fatally.

Dr. A. O. Speedy, one of the medical officers of a large dispensary district in one of the poorest localities of the city, treated nearly 400 small-pox patients at their own homes. Not one of these had been re-vaccinated. Dr. Thomas Purcell, another of the poor-law medical officers, who treated several hundred cases of small-pox, did not meet with a single case after re-vaccination.

The statistics of small-pox and vaccination relative to the present epidemic of small-pox which we have collected plainly show that vaccination is unquestionably a protection against small-pox. It is idle

to assert that persons thoroughly vaccinated enjoy perfect immunity from this disease: all that can fairly be claimed for vaccination is that it greatly lessens that "receptivity" which appears to be a factor in contracting the disease.

"The experience of the public schools in the city of New York, the past year has shown that in the midst of an unusually severe and widespread prevalence of small-pox, and which attacked upwards of 2,000 persons in more than 1,400 houses, the 240,000 school-children of the city have remained so secure from the contagion that scarcely a death, and only a very few cases of the disease, have occurred. Not a case, indeed, has been ascertained in which a child, with approved vaccination, has died of the disease out of this vast number of pupils. But in a single ragged school, containing 150 pupils, only 50 of which were found to bear any mark of vaccination at the first inspection, more cases of small-pox occurred in a single fortnight after the first case, than have occurred in all the well-vaccinated school-children in the city in a year."—*Bulletin of the New York Academy of Medicine, No. in 1870.*

In his annual report on the health of Liverpool for the year 1871 Dr. Trench traces the recent epidemic of small-pox in that city to the arrival of a ship from Galicia. Two Spanish sailors had sickened at sea with small-pox in this ship, and on her arrival at Liverpool they were sent to hospital. From these patients the contagion appears to have been propagated throughout the city; but the supply of contagion was kept up by fresh arrivals of infected ships. At first 30 per cent. of the cases of small-pox were amongst seamen. The influence of vaccination as a prophylactic was clearly shown in Liverpool. Amongst the cases of small-pox where there was doubtful vaccination the mortality was 56·4 per cent.; where the patients had one visible cicatrix the deaths amounted to 14·9 per cent.; where two cicatrices were visible the mortality was 9·8 per cent.; and lastly, in the case of those who had three cicatrices, the deaths were at the rate of 7 per cent.

The *Lancet* for March 30th, 1872, contains a report by Dr. J. Harris Ross, on the recent epidemic of small-pox in Brighton. 271 cases were treated in the Workhouse Hospital, and careful notes were taken of 258 of them: of these 105 were unvaccinated and 153 were vaccinated. The mortality was 28·9 per cent. amongst the unvaccinated, and 1·99 amongst the vaccinated patients. 146 unvaccinated patients were treated at their homes, and of these 38·3 per cent. died. Of 247 post-vaccinal cases treated at private houses, 32 (12·9 per cent.) proved fatal.

It would appear that re-vaccinated persons are less liable to contract small-pox than persons of the same age who are only protected by primary vaccination. This to a great extent may be due to the second operation being more carefully performed, for it is generally conceded that some years ago vaccination was often very imperfectly carried out. Since the early part of 1870 many thousand persons have been re-vaccinated in Dublin, and yet we rarely hear of re-vaccinated persons contracting variola, although at least 3 per cent. of the whole population has suffered from this disease. There are several thousand soldiers stationed in Dublin, and there has been only a few cases of small-pox amongst them, although they are of an age which is favourable to the reception of the disease. No doubt the exemption of these soldiers from the epidemic is to a great extent due to the fact that they are, with few exceptions, re-vaccinated.

We learn from the *Gazetta Medica Lombardia* for February 17th, 1872, the following statistics relative to vaccination and small-pox in Milan during the year 1871:—

The total number of vaccinations performed under the auspices of the Milan municipality ("animal" vaccine virus being employed), between January 1 and December 31, 1871, was 17,069. Of these 1,504 were vaccinations of children, of which 1,270 were successful in their results, 4 were spurious, 35 failed, and 195 were not verified. There were also 15,565 adults re-vaccinated, the results being successful in 5,039, spurious in 435, and unsuccessful in 3,814, while in 6,277 they were not verified. Among these 15,565 re-vaccinations there were only 68 cases in which variola in a mild form, or varioloid, appeared.

The following statistics relative to small-pox in Hamburgh—where the disease was very prevalent in 1871—have been officially published, viz.:—3,301 vaccinated persons took small-pox, and of these 347 died, and 2,954 recovered. On the other hand, of 710 unvaccinated persons who suffered from small-pox, 700 died and 10 recovered! If these figures are reliable, who, in future, can doubt the efficacy of vaccination?

PART IV.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE
COLLEGE OF PHYSICIANS.

Wednesday, April 17, 1872.

DR. BEATTY, in the Chair.

DR. LYONS'S *Report on Small-pox in the Hardwicke Hospital for the year beginning April 1st, 1871, and ending March 31st, 1872.*

MR. CHAIRMAN,—I am about to bring under the notice of this Association some statistical account of the small-pox cases that have been treated during the present epidemic in the House of Industry Hospitals. I wish it to be understood that even now—although on a former occasion I asked the indulgence of this association to defer my paper—I do not wish it to be considered that those statistics are to be taken as final and complete. We all know the great difficulty of working statistics to a final conclusion during the prevalence of an epidemic, and at a period when some of the cases which are the subject of consideration in the paper may be said to be still *sub judice*. My first intention was to present to the Association a statistical account of 500 cases. Since the last occasion on which the Association met 111 additional cases have occurred under our observation, and as that brings the statistical account within our hospital year—namely, from the 1st April, 1871, to the 31st March, 1872—I think it better to take that number, and discuss the statistics of all the cases admitted for an entire year. This has entailed on me a large amount of additional labour. It will also be to some extent an apology for the

results not being finally complete, as I should wish them to be before presenting them to this distinguished Association.

I shall not pretend that these statistics are to be taken as a final exposition of the conditions under which the epidemic has presented itself on the present occasion. I have no desire to push beyond proper limits my idea of the value of statistics; but I think it will be conceded that it is only by a statistical inquiry we can gauge at any given period of an epidemic some of the most important problems which affect public health in connexion with it. A statistical inquiry upon a reasonably large number of cases, during the prevalence of an epidemic, may be considered as a mode of heaving the medical log, ascertaining how the ship of public health is making way, what the rate of mortality in the epidemic is, what the mortality is in relation to other epidemics in present or past times; what in fact may be the gravity of the conditions in which the population is placed by reason of the epidemic visitation, and how long such epidemic is likely to last; to what extent it is likely to spread through the population; what means the public may be called upon to employ to check its spread and diminish its mortality, and what lessons may be learned from our experience of one epidemic towards the prevention of another.

In speaking of the Hardwicke Hospital it is worth observing that it was erected in the year 1803, on a tolerably good site, with its front facing the south, which gives it all the advantage of proper insolation; that it is divided into six wards, each of which has windows at both sides; and that its shape is that of the letter T, so that every portion of the wards in the building is exposed to full perfilation by currents of air, and to free insolation or exposure to the sun. Besides the ordinary hospital accommodation the Board of Governors, with great spirit and much liberality, have added extra wards by converting the convalescent building into wards for the reception of small-pox cases, and thus largely increasing the accommodation. A ward has also been specially set apart for the reception of the resident clinical clerks if they should be affected by the disease. Unfortunately the vast expense incurred by the increased number of cases thus provided for and treated in the institution has not for the present added to its financial prosperity.

The Records of the Hardwicke Hospital may be taken as an index of the status of diseases as to endemic and epidemic visitations in this city and the surrounding districts, at any given period, with some tolerable approach to accuracy.

The following brief statement will show the summary results of the admissions under the head of small-pox for the last thirty years:—

Return of Small-pox Patients admitted into Hardwicke Hospital from 1841 to 1871, commencing 1st April, ending on 31st March each year.

Period	Total number admitted	Males	Females	Result		Vaccinated	Not Vaccinated	Unspecified	Ages					
				Recovered	Died				Under 5	5 and under 15	15 and under 20	20 and under 40	40 and under 60	60 and under 80
1841-2	2	—	2	2	—	—	—	2	—	—	1	1	—	—
1842-3	33	10	23	25	8	—	—	33	3	11	7	12	—	—
1843-4	6	3	3	5	1	1	1	4	—	2	1	3	—	—
1844-5	6	3	3	6	—	—	1	5	2	1	—	3	—	—
1845-6	85	45	40	64	21	—	—	85	19	29	18	19	—	—
1846-7	27	16	11	19	8	—	—	27	3	9	9	6	—	—
1847-8	21	13	8	19	2	—	—	21	2	5	6	8	—	—
1848-9	63	38	25	50	13	—	—	63	6	29	15	12	1	—
1849-50	155	83	72	100	55	14	12	129	11	52	37	53	2	—
1850-51	40	19	21	20	20	24	10	6	3	11	13	13	—	—
<i>TOTAL,</i>	<i>438</i>	<i>230</i>	<i>208</i>	<i>310</i>	<i>128</i>	<i>39</i>	<i>24</i>	<i>375</i>	<i>49</i>	<i>149</i>	<i>107</i>	<i>130</i>	<i>3</i>	<i>—</i>
1851-52	39	17	22	27	12	19	18	2	6	7	14	12	—	—
1852-53	93	42	51	65	28	48	40	5	10	22	23	38	—	—
1853-54	54	23	31	41	13	13	40	1	8	29	5	12	—	—
1854-55	131	64	67	107	24	62	63	6	24	28	35	44	—	—
1855-56	30	17	13	18	12	—	2	28	11	5	3	11	—	—
1856-57	30	20	10	18	12	—	—	30	10	9	4	7	—	—
1857-58	15	7	8	12	3	1	—	14	1	4	3	6	1	—
1858-59	80	38	42	71	9	—	—	80	10	23	9	36	1	1
1859-60	72	35	37	62	10	—	—	72	7	15	14	28	8	—
1860-61	15	10	5	14	1	—	—	15	3	3	2	5	2	—
<i>TOTAL,</i>	<i>559</i>	<i>273</i>	<i>286</i>	<i>435</i>	<i>124</i>	<i>143</i>	<i>163</i>	<i>253</i>	<i>90</i>	<i>145</i>	<i>112</i>	<i>199</i>	<i>12</i>	<i>1</i>
1861-62	5	2	3	4	1	—	—	5	—	1	—	4	—	—
1862-63	2	2	—	2	—	—	—	2	1	—	1	—	—	—
1863-64	16	8	8	15	1	—	3	13	—	—	—	—	—	—
1864-65	131	70	61	120	11	52	27	52	—	7	6	3	—	—
1865-66	100	56	44	87	13	62	33	5	7	55	24	45	—	—
1866-67	36	15	21	30	6	23	8	5	1	25	29	45	—	—
1867-68	2	—	2	2	—	1	1	—	4	12	7	11	2	—
1868-69	—	—	—	—	—	—	—	—	—	1	—	1	—	—
1869-70	4	3	1	3	1	—	—	4	—	—	1	3	—	—
1870-71	3	1	2	3	—	2	—	1	—	1	1	1	—	—
<i>TOTAL,</i>	<i>299</i>	<i>157</i>	<i>142</i>	<i>266</i>	<i>33</i>	<i>140</i>	<i>72</i>	<i>87</i>	<i>13</i>	<i>102</i>	<i>69</i>	<i>113</i>	<i>2</i>	<i>—</i>

SUMMARY

Total number of admissions—	}	1,296	{	660 Male. 636 Female.
1841-1871.				
Result—Recovered - -	}	1011 285	}	1,296—21.99 per cent.
Died - - -				
Vaccinated - - -	}	322 259 715	}	1,296.
Not vaccinated - - -				
Unspecified - - -				

AGES.					
Under 5 years,	-	-	-	152	
Over 5 and under 15	-	-	-	396	
„ 15 „ 20	-	-	-	288	
„ 20 „ 40	-	-	-	442	
„ 40 „ 60	-	-	-	17	
„ 60 „ 80	-	-	-	1	
80 and upwards	-	-	-	0	
Gross total,				1,296	

It will be in the recollection of many of the senior members of the Association that small-pox was for many years fatally prevalent in Ireland. It will be seen that the statistics of the Hardwicke Hospital show a general accord with those of the Poor Law and other returns for the past three decades:—1841–1851, 1851–1861, 1861–1871.

The Census returns show that in the ten years preceding the 6th June, 1841, the deaths from small-pox were 58,006. In the ten years' period, ending 31st March, 1851, 38,275; and in the ten years' period to 7th April, 1861, 12,727, namely, 6,714 males, and 6,013 females.

We now approach that period in which, owing to the extension of vaccination and the wise sanitary measures adopted by our brethren in the dispensary districts throughout the country, the population was brought to that condition of protection that the disease may be considered to have practically disappeared from our records. In the year ending 31st March, 1869, no case of small-pox was admitted into the Hardwicke Hospital. On June 17th, 1869, one case, a male, was admitted from James's-street. On September 3rd, 1869, another case, Mary Farrell, was admitted from Montpelier Hill. In the return of the Register-General for the quarter ended the 31st December, 1869, we find that but one death from small-pox was registered in Ireland. That death occurred in the Hardwicke Hospital under the following circumstances.

On Sunday, November 7th, 1869, while paying my usual visit to the hospital, I was asked by the pupils and by one of the most experienced nurses to come at once to see a case of a most extraordinary form of disease, which had just been admitted, and which no one had up to that moment apparently diagnosed. I found in the ward a Swedish sailor, who had just been brought in, and assisted into his bed by the captain of his ship, and some of his comrades. His name was Sweyn Johanson. He was a native of the province of Smoland in Sweden, and his comrades claimed for him, with no little pride, the affinity of birth in the same district with the celebrated Christine Nilsson, the eminent cantatrice. The case was one of the most aggravated and appalling form of the variola corymbosa. His captain and comrades stated that they were

entirely unaware of any small-pox cases existing in the port at the time they took him on board, and that he was fully fourteen days at sea before he became sick. They had no idea of the nature of his case. No one else in the ship sickened, and although the case turned out to be one of the most aggravated form of not only confluent but corymbose small-pox, but one individual could in any manner be traced as having been contaminated by his presence in the ship, or in the hospital. He died four days after admission.

Though the clinical clerk immediately in charge of him did not sicken, Mr., now Dr. Brady, my clinical resident, who had no special relations with this patient, sickened of very modified small-pox, on December 7th, 1869, subsequently. He made a good recovery. I make a present of this case handsomely to the contagionists and importers. But from the number of persons, comrades in his ship, fellow-patients, nurses, physicians, clinical attendants, and students who were in contact with this patient, and who escaped all infection, we must conclude that some other, although unknown, conditions preside over the state of receptivity of disease in any given population, and that the introduction of even the most virulent form of a disease admittedly contagious, is not in itself sufficient to cause the spread of the malady in epidemic form amongst a given population, unless that population is in a state to receive and propagate the disease. Whether this condition of resistance to, or capacity for the reception and propagation of disease, be dependent on meteorological or terrene conditions, or on special states of the constitution of the population at the time, in the place, and under circumstances to be given, I am not prepared to state; but of this I have myself no doubt, that, so to speak, certain states of inflammability of the constitution of the populace exist at particular periods, when, on the approach of the exciting stimulus of contagious epidemic disease, it will break forth in full fury, and on the other hand, particular passive states of the popular constitution exist, when even in the presence of undoubted examples of the most virulent diseases, at other times epidemic, the local constitution refuses to take fire, and contagion is inert.

The Hardwicke Hospital records show that from this date a considerable period elapsed during which no small-pox cases were admitted.

On the 4th April, 1870, William Knowles was admitted from the Richmond Lunatic Asylum (discharged cured). On the 27th July, 1870, Richard Hynes was admitted from Maher's-court, North Brunswick-street (discharged cured).

We now come to the first case which our entries of the present epidemic record. The case is peculiar in its history, and not less important, and as it has been erroneously stated to have been imported, I must be excused for giving the exact details in regard to it. This patient was named Anne Cox. She was a servant girl, aged 18,

unmarried; she had resided with her mistress at Suffolk-street, Dublin, until a short time previous to her admission to hospital. On account of her mistress's health she went with her to Amlech in N. Wales. When there a few days the mistress fell ill, and developed small-pox, but apparently in a very modified and mild form, for she was able to be up and to assist in nursing Anne Cox, who fell ill very soon after, if not about immediately the same time as her mistress.

Whether the girl Cox's initial symptoms were more severe than those of her mistress I cannot say, but she was speedily deported to this city; her arrival was heralded by telegraph, a guard of honour of police was told off to wait her arrival, and she was speedily transferred from the inhospita terra of the Cymri to the hospitable walls of the Hardwicke.

Anne Cox was admitted on the 28th February, 1871, and not discharged until the 25th April. No one in hospital, or elsewhere, so far as we know, took the disease from her, and the next entry is that of Christopher Doran, aged one year, from Montgomery-street, Dublin, on 17th April, 1871. No fresh case was admitted till 1st May, 1871. This month, May, 1871, presents in all but 13 admissions; June also shows but 13 admissions, with 1 death from small-pox, the first on June 29, being a female, aged 33, not vaccinated, and confluent; July, 19 admissions; August, 30 admissions; September, 31 admissions. October first brought us full face with the conviction that an epidemic of some magnitude was upon us, when the admissions reached 69, and the deaths amounted to 10.

I may now state the dates of admissions in regard to the 611 cases, the statistics of which I propose to discuss.

The girl, Anne Cox, was admitted on 28th February, 1871; her case therefore is included in the record of 1870-71. The 611th case was admitted on 30th March, 1872. The 31st March was Easter Sunday, and no persons sought admission on that day. The admissions per month were as follows, which will show the gradual rise of the epidemic:—

SMALL-POX CASES.

ADMISSIONS PER MONTH TO THE HARDWICKE HOSPITAL.

	Cases.	Deaths.	Percentage of Fatality.
1871.—January .	. 0	0	—
„ February 28th .	. 1	0	—
„ March .	. 0	0	—
<hr/>			
„ April .	. 1	0	—
			2 G 2

1871.—May	.	13	0	—
„ June	.	13	1	7.69
„ July	.	19	3	15.78
„ August	.	30	5	16.66
„ September	.	31	7	22.58
„ October	.	69	10	14.49
„ November	.	86	14	16.27
„ December	.	97	21	21.64
1872.—January	.	103	25	24.27
„ February	.	56	13	23.21
„ March	.	93	21	22.58
		611	120	19.63 average.

Of the 611 admitted within the dates specified, April 1st to March 31st, a total of 120 deaths is recorded. From these must be deducted one, who, being ill of phthisis when attacked, recovered of the small-pox, and died of the lung disease soon after, which gives the total mortality 19.47 per cent. If we deduct the purpuric cases and those admitted moribund, about 40 in all, the net mortality is about 13 per cent. left on the total admissions. The confluent cases were in all 203. Of the total admissions, 611 in number, 541 were found to have been vaccinated, 66 bore no marks of vaccination whatever, 4 were doubtful, and at least 3 had been re-vaccinated.

Three had been born in hospital, one being the child of a non-variolous mother, took the disease and recovered; one was born dead of a mother re-vaccinated, and who had the disease in a very modified form, but the foetus (of five months), exhibited numerous spots, and appeared to have died of the virus of small-pox.

It will be seen that the percentage mortality rose steadily from June to September. In October, though the total number of cases admitted was more than twice as great as in September, the percentage mortality sank to 14.49, and thenceforward steadily rose, month by month, till January, when the mortality for all cases admitted reached the very high proportion of 24.27, considerably above the average on the total mortality, which is 19.47 per cent.

This rate of mortality shows that to a certain extent we have succeeded in the management of the disease, and that the present epidemic is less mortal than small-pox has been in previous years. I have the figures from the London Small-pox Hospital for the period of ten years, extending from 1841 to 1850, from which I find that the total number admitted was 3,422, and that the deaths were 734, or 21.44 per cent. Therefore if we compare our present epidemic, on the data that the Hardwicke Hospital furnishes, we find the mortality less as compared

with the past 30 years, and as compared with the London epidemic in the proportion of 19·63 to 21·44.

The following figures will show the distribution of the mortality :—

	Died.	Per Cent.
Of the re-vaccinated (3 in all)	0	
Of the 541 vaccinated 60, all confluent,	11·82
of whom 3 were also purpuric 4, not confluent,	
of whom 3 were also purpuric	
Of the doubtfully vaccinated (4 in all)	. 2	
Of the 66 non-vaccinated,	{ 43, confluent	78·57
	{ 10, not confluent	
	<hr/> 119	

The mean mortality was 11·82 per cent. of those vaccinated. Of the non-vaccinated, including the doubtful cases, the mortality was as high as 78·57 per cent., which is the most powerful argument that can possibly be adduced in favour of the salutary influence of vaccination. Our mortality in this respect differs from the recorded mortality in other epidemics. Thus on the total admissions into the London Small-pox Hospital for ten years the average mortality was 21·44 per cent.; of vaccinated with cicatrices the mortality was only 7 per cent.; and of the total unvaccinated the mortality was only 36 per cent. The severity of the cases admitted to the Hardwicke Hospital may be judged from the above data.

To put it in plainer language, and in more striking terms, of the non-vaccinated only 13 out of 66 recovered, or in the proportion of only 19·69 per cent., while of those protected by vaccination, 474 out of 541, or in the proportion of 87·61 per cent., recovered.

I have stated that the confluent cases were 203 in all. Of these it will have been seen that 60 vaccinated and 43 non-vaccinated cases, or 104 in all, died confluent, being a little more than half, or in the exact proportion of 51·23 per cent. Confluence was therefore not the determining cause of death in all instances, though undoubtedly it was a powerful factor.

Of the vaccinated cases which died, 64 in number, 60 died confluent; but of the 53 deaths of non-vaccinated cases 10 were *not* confluent.

We may conclude then that confluence is not so essentially the fatal element as has been often supposed.

The most striking fatality attended the cases which were marked by any considerable degree of purpuric complication. Of such cases we may recognize three degrees.—(a) Purpura with well developed small-pox, going through all its stages, not usually fatal. (b) Purpura in excess of small-pox. (c) Purpura without small-pox pustules. Of such

cases, at least, 20 in all were admitted, and of these all proved rapidly fatal.

Age.—We may now proceed to consider the epidemic in its operation, as exhibited upon the two sexes, and at the various periods of ages. The sexes, if judged by our admissions, were attacked in somewhat unequal proportions, 329 males with 71 deaths, and 282 females with 48 deaths.

Ages of 611 Cases of Small-pox admitted to Hardwicke Hospital from April 1st, 1871, to 31st March, 1872.

Age	Number Admitted	Deaths	Per cent.	Age	Number Admitted	Deaths	Per cent.
*0 year	5	1	20.0	36 years	4	1	25.00
1 "	1	—	—	37 "	1	—	—
2 "	1	1	100.0	38 "	3	—	—
3 "	2	—	—	39 "	1	—	—
4 "	1	1	100.0	40 "	14	2	14.23
5 "	8	4	50.0	41 "	1	1	—
6 "	5	3	60.0	42 "	2	—	—
7 "	6	1	16.66	43 "	—	—	—
8 "	9	—	—	44 "	1	1	—
9 "	9	1	11.11	45 "	—	—	—
10 "	21	2	9.52	46 "	—	—	—
11 "	14	5	35.71	47 "	—	—	—
12 "	20	4	20.0	48 "	2	1	—
13 "	18	—	—	49 "	—	—	—
14 "	30	4	13.33	50 "	2	2	—
15 "	28	4	14.28	51 "	—	—	—
16 "	44	3	6.81	52 "	—	—	—
17 "	38	1	2.63	53 "	—	—	—
18 "	49	7	14.28	54 "	—	—	—
19 "	29	6	20.68	55 "	2	2	—
20 "	41	10	24.39	56 "	—	—	—
21 "	26	6	23.06	57 "	—	—	—
22 "	28	9	32.17	58 "	—	—	—
23 "	26	3	11.53	59 "	—	—	—
24 "	23	3	13.05	60 "	—	—	—
25 "	16	6	37.50	61 "	—	—	—
26 "	12	4	33.33	62 "	1	—	—
27 "	9	1	11.11	63 "	—	—	—
28 "	15	8	53.33	64 "	1	—	—
29 "	1	—	—	65 "	1	—	—
30 "	18	2	11.11	66 "	1	1	—
31 "	1	1	100.0	67 "	—	—	—
32 "	4	1	25.0	68 "	—	—	—
33 "	8	2	25.0	69 "	—	—	—
34 "	4	4	100.0	70 "	—	—	—
35 "	4	1	25.0				

* Under 1 year.

These figures present a very remarkable contrast with the results of our former experience in this hospital, in so far as that in the admissions for 30 years the males were 660, and the females 636.

In considering the epidemic invasion by the ages of those attacked, we come upon some facts worthy of notice.

The mean lethal force or coefficient of mortality we find to have been for all our entries 19·63 per cent.

The seizures are very variably distributed over the different lustra of ages, or 5 year periods, and there are shown very singular differences in fatality, when the single years of all are separately viewed as in the annexed tables.

The following summary shows the admissions, deaths, and per-centage of mortality at the quinquennial periods of life:—

	Admitted.	Deaths.	Per-centage mortality.	Mean.
Fœtus in utero, mothers admitted pregnant,	6	1	16·66	15·32
Under 1 year,	5	1 ^a	20·0	
1 to 5 years,	13	6	46·15	
5 to 10 „	50	7	14·0	
10 – 15 „	110	17	15·45	24·77
15 – 20 „	201	27	13·43	
20 – 25 „	119	27	23·52	
25 – 30 „	55	15	27·27	
30 – 35 „	21	9	42·85	57·14
35 – 40 „	23	3	13·04	
40 – 45 „	4	2	50·00	
45 – 50 „	4	3	75·00	
50 – 55 „	2	2	100·00	
55 – 60 „	—	—	—	
60 – 70 „	4	1	25·00	
	611	120		

Careful inspection of the above figures will show that so far this has been a *young* epidemic, or rather an epidemic amongst the young, swooping upon and cutting down the very flower of the population.

Thus of the 611 cases recorded 379 were of and under 20 years of age, of whom 58 died, or in the proportion of 15·30 per cent.; 218 were between 20 and 40, of whom 54 died, or 24·77 per cent.; 14 only were of ages between 40 and 70, of whom 8 died, or in the proportion of 57·14 per cent. Of the 120 deaths 112 were of persons at and under 40 years of age, of whom a fraction over half were at and under 20 years of age.

Taken year by year, the figures representing admissions and deaths are, in many instances, too small to admit of any legitimate conclusions being drawn from them, but the results are so singular at some of the

^a 4 months old.

early periods of life, that at the risk of appearing prolix, I give the returns of age year by year. They may serve as a means of comparison or illustration for other statistical records, by other gentlemen connected with this Association, from whom we have reason to expect very important contributions to the history of the present epidemic.

Though imposing on others the same cautions I do upon myself with regard to these figures, I may yet be permitted to point to the comparatively lighter mortality amongst children under 1 year to the very heavy mortality of 50 per cent. amongst those at 5 years of age; to the comparatively small mortality at 10 years, only 9·52 per cent.; to the very heavy mortality at 19, 20, 21, and 22 years; to the still weightier fatality at 25, viz., 37·50 per cent., and at 26, 33·33, and to the severe incidence of the epidemic as a fatal malady at the ages between 31 and 40 inclusive, namely 27·27 per cent.

The epidemic has not been with us as young an epidemic as it has been at former periods and in other places. Thus in the London epidemic, of 9,762 who died of small-pox in England in 1837-38, 7,340 were under 5 years of age—a far larger proportion than our hospital returns show; 1,668 were between 5 and 15; 528 were between 15 and 30; 210 were between 30 and 70; 16 were upwards of 70. Of 2,285 deaths in London from small-pox in 1840-41, 2,060 were under 15 years of age, showing a very remarkable mortality in that institution at those very early ages, and far greater than the mortality of similar ages in this epidemic, so far as can be judged of by the records of one hospital.

The next point I shall consider is the area of supply represented by the cases admitted into the Hardwicke Hospital. From the north side of the city 404 cases were admitted; from the south side, 130; from country districts, 32; Southampton, 1; North Wales, 1; ship, 1; Italian sailors, 3; police, 24; Richmond Lunatic Asylum, 15. This will show that our cases represent very fully the general distribution of the epidemic in the Dublin districts, including certain districts within a range of 10 to 15 miles outside Dublin.

Sex.—The admissions and deaths by sexes are found to be as follows:—Of 611 admitted 329 were males and 282 females. The deaths amongst the males were 71, or in the proportion of 21·58 per cent., while the deaths amongst the females were only 48, or in the ratio of 17·02 per cent. In trying to fix the probable determining cause of the greater mortality amongst the males, we must refer to the actual ages of the males who died as contrasted with those of the females, and the mean age of the admissions in both instances.

The mean age of the females who died was 19·77, that of the males 20·66. The mean age of the males admitted was 21·49, while that of the females was only 18·24. The aggregate age of the 329 males was 7,073; that of the 282 females 5,145.

We have already seen that in discussing the gross mortality, the deaths at and under 20 were very sensibly less than the deaths at the ages from 20 to 40.

It will be observed that the aggregate and mean age of the males admitted to the Hardwicke Hospital being well over 20, threw them into the category of the maximum risk, while the females in aggregate and mean age are shown to be far within the period in which the maximum of safety is enjoyed by those who have the misfortune to contract the disease. It is a matter of profound interest in a physiologico-pathological point of view to consider what agencies in the system are in operation before and after the 20th year of age which may determine the coefficient of mortality in this dire disease. It is also a matter for inquiry whether the records of other institutions will show a concurrence in these statistical particulars as to the greater incidence of mortality at particular ages, and in the sexes respectively. For the present I shall content myself by pointing to the facts as observed in the statistics I have had the honour to lay before you.

Cases in which Small-pox was contracted in discharge of Duty.—I shall now turn to an interesting and important point—the tendency of the disease to propagate itself amongst persons in contact with the sick. Of the staff of the institution, including physicians, surgeons, apothecary, resident officers, resident and clinical pupils, nurses, ward-maids, porters, and others, numbering in all 157. The following exhibited the disease during the present epidemic. Of the total number of the officials it is necessary to state that some 50 individuals in all had more or less actual personal relations with the small-pox patients in the Hardwicke Fever Hospital. As the service of the three establishments—the Richmond, the Whitworth, and the Hardwicke—is practically distinct, the attendants in the two former had no contact with small-pox patients:—

	Total Number	Number Attacked	R E S U L T	
			Died	Cured
Medical Officers, - -	4	0	0	0
Resident Clinical Clerks, -	7	2	0	2
Clinical Pupils, Visitors, &c., -	100	5	0	0
Nurses and { Whitworth, -	12	5	2	3
Ward Maids, { Hardwicke, -	20	1	0	1
Laundry Maids, - -	8	0	0	0
Gate Porters, Messengers, &c.,	6	1	0	1

Of the clinical clerks both were vaccinated. Of the ward-maids in the Whitworth, one was unvaccinated. She died of secondary fever six days after removal to the Hardwicke Hospital. The case was confluent in a marked degree. Another ward-maid, also from the Whitworth Hospital, and with good marks of vaccination, died of the confluent form of the disease. Of the nurses and ward-maids in the Hardwicke who were exposed to the fullest extent to the contagion of the most virulent forms of the disease by almost hourly contact with the sick, only one individual—a ward-maid—took small-pox. She had been vaccinated and made a good recovery.

It is an important fact that not one of the individuals connected with the large laundry establishment in the institution took the disease. We constantly hear stories of individuals taking the disease by washing clothes. The clothes of the patients were washed in the establishment, and no individual connected with the laundry took the disease. One ward-maid only of all those in personal contact, day and night, with the sick in the Hardwicke Hospital took the disease, and she recovered from it. It is a very remarkable fact that five ward-maids in the Whitworth Hospital, who had no relation whatever with the Hardwicke Hospital, developed the disease. I attribute this to their sharing in the general epidemic tendency with other members of the population at large, and not to any personal contact, which was perfectly impossible, and cannot be adduced to account for their getting the disease. In estimating the power of contagion in the propagation and dissemination of the disease, the conclusions to be drawn from the foregoing data cannot be overlooked. One only of the female attendants, constantly about the sick, took the disease; whilst of those attendant upon non-contagious cases in the adjoining hospital, five developed the disease, of whom two died. It is obvious that some agency for the development and propagation of the disease is requisite other than that of transmission *per contactum* from one infected individual to another.

Of patients in the Whitworth Hospital—a completely detached building, and separated by a public highway from the Hardwicke Hospital—nine individuals developed small-pox. Of these only one died. It is worthy of note that this case was an example of very chronic and very inveterate general psoriasis. One other case, as already recorded, who was an inmate of the Whitworth, and labouring under chronic phthisis, developed small-pox, but convalesced from that disease in an unequivocal manner, and died some weeks subsequently of the original ailment.

Of patients in the Hardwicke convalescing from other febrile diseases, the following number developed small-pox, but, without exception, all of them recovered:—

Vaccinated cases,	-	-	39
Unvaccinated,	-	-	4
			—
Total,			43

Day of Death.—An important element for examination is the day of death. In appreciating this point we have to consider the period which elapsed prior to admission to hospital, and the number of days of residence in hospital before the fatal issue took place.

From a minimum of 2 to a maximum of 12 days elapsed before the male cases which proved fatal were admitted into hospital, the mean being 3·72 days. In the case of the females from a minimum of 2 to a maximum of 15 days elapsed before the cases were admitted, the mean of all being 4·71 days. The deaths in males occurred at a minimum period of from one day, or, in some instances, part of a day or night, to a maximum of 24 days. The mean day of death, or of sojourn in hospital, for males after admission is found to be 7·24.

In females the deaths occurred at a minimum period of one day, or part of a day, to a maximum of 16 days after admission, except in the instance of the phthisical patient, who died 28 days after admission with small-pox. The mean day of death, or sojourn in hospital, for the fatal female cases was 5·64 days. This would seem to show, when taken in contrast with the rates of mortality in females and males, that while fewer females per cent. to admissions died, those who did die died comparatively more rapidly, their endurance not being so great as that of the males when fatally struck by the epidemic.

Proximate Cause of Death.—In considering the immediate pathological cause of death in this disease we are met with many difficulties. Few, if any, cases can be said to have died of any single predominant complication, if we except the purpuric cases, which seemed to respect neither age nor sex, and to be in no degree influenced by previous vaccination.

Cases with laryngeal complication were more or less associated with bronchial affections, and were also in many instances examples of a highly confluent form of the disease, in which no other than a fatal issue could be anticipated.

Of the 120 deaths to be accounted for, 20 died mainly of purpura hæmorrhagica, about in equal proportion males and females.

Ten females and five males—in all 15—died of the intensity of the primary fever. In equal proportion males and females, 25 died of pharyngo-laryngeal complications, involving the trachea also in most cases. 20 males and 10 females—in all 30—died of secondary fever. The remainder, in about equal proportion male and female—in all 29—died of secondary bronchial effusion; as already stated 1 died of phthisis.

Duration in Hospital.—An interesting point is to discuss what was the main duration of the several cases in the hospital—a point that has a

very important public and financial bearing. Those 611 cases occupied 10,840 hospital days. Any one familiar with the mode of dealing with statistics, in prisons and other large institutions, will know that the most convenient method of discussing this part of the question is by reducing the total admissions and period of time to what is called either prison days or hospital days. The mean average sojourn in the hospital was 17·74 days.

Modes of Recovery.—I propose now, in the very briefest manner, to allude to an important consideration—namely, the modes of recovery from the disease: how did those patients recover who did recover? Many of them recovered in a remarkably quick manner. It has been no uncommon thing to find a patient who came in one day in a condition of great peril, convalescent by that day week. This is a very important point. The cases which recovered most rapidly were those in which the eruption came freely to the surface, in which there was free pustulation, and in which the whole of the matter formed was thrown to the surface, crusted rapidly, and scaled off in very extraordinary amount, leaving a mass of scales in the bed almost in the same way as in the cases of psoriasis we are so familiar with in hospital. I was sorry to see the risible faculties of this association called into play when my friend Professor Smith stated that he considered that a main feature of this disease was a formation of pus coming freely to the skin, which should be encouraged as much as possible, and that one of the favourable conditions of the disease was throwing pus freely to the surface. It is my observation that a large number of cases recover rapidly and with the least possible ill consequences which exhibit free pustulation, which comes largely to the surface, and which scales off in the manner I have mentioned. In fact the patients appear as if undergoing a process of changing the skin, and as if the emunctory action were performed entirely through the cutaneous surface.

Purpuric Form of the Disease.—Of this formidable type of small-pox, a very large proportion of cases was presented.

It may be stated that in all at least 100 cases exhibited some form of purpuric or hæmorrhagic complication.

As much want of precision prevails in regard to this term, I propose to class under three heads the chief examples of small-pox, with purpuric eruption, hæmorrhagic complication, or both combined.

1. That form in which, with a well developed small-pox eruption, more or less numerous, purpuric spots or patches became developed on various parts of the body, and in which hæmorrhage took place from the gums, angles of mouth, nose, lungs, stomach, bowels, kidneys, or vagina.

In a certain proportion of these cases, the small-pox eruption receded when the hæmorrhagic and purpuric state became developed, and such cases almost invariably proved fatal.

In other cases, notwithstanding even considerable hæmorrhages, and free purpuric spotting, the small-pox patches ran their course fully, and convalescence followed. In this class of cases, however, hæmorrhage sometimes proved a fatal complication. A notable instance was that of a lad, aged nine, in whom immense thickening and protrusion of the lips and buccal membrane occurred; constant weeping of blood in large quantity took place at the angles of the mouth, and death speedily resulted. A similar affection occurred in a man, aged thirty-three, who, however, had in addition numerous purpuric blebs on various parts of the body; both these were unvaccinated cases.

2. In the next class of cases, the purpuric and hæmorrhagic conditions were from the first far more prominent features of the disease than the variolous eruption. These cases were largely fatal, but not universally so.

3. In the third class of cases, purpura was the main disease present, and the small-pox eruption was either very sparingly developed in the earliest stage of hardened tubercles in the skin, or as in extreme and rapidly fatal cases was not developed at all. Of such cases twenty-one in all are on the record, and of these twenty proved fatal. Of the twenty fatal purpuric cases, fourteen occurred in persons with vaccination marks, and six in persons not protected by vaccination.

I have hardly in my life ever seen instances of more striking disease than in some of those purpuric cases. I will in the briefest possible manner mention the details of a few. One of them was a patient about thirty years of age, a fine, strong, powerfully built young man. It is remarkable, and, I believe, consistent with the experience of all other observers, that the purpuric form is generally worst in persons of a full and well developed habit of body. It is not my experience that it is at all worse in persons who had been addicted to drinking and free living before hand. No such thing could be suspected in the case I am about to mention. This man came in with that singular condition of dark red colour of the skin, which would make one hesitate for a moment as to whether the disease was really scarlatina or about to be small-pox. We now know, however, in this epidemic that those cases become rapidly fatal, and that if they survive any time they develop small-pox, and not scarlatina. This patient like many others, retained full consciousness up to a short period before his death, and what is also very remarkable there was no very great disturbance in the circulation; his pulse did not rise high. In another very singular case within a few hours of death, I found the faculties perfect, the mind free, and the pulse only seventy. This man died in a very sudden and unexpected manner. In the *post-mortem* room, we found no trace whatever of pustulation. The surface of the skin was of a dark mulberry hue, verging on blue-black. The lips were swollen and everted in an extraordinary degree. The conjunctival membrane was everted in the most singular manner I have ever seen; the

eyelids both above and below were completely turned out, and converted each into a large prominent mass filled with blood, the conjunctival membrane being external, and the cutaneous envelope turned up and down in either case. In another remarkable instance where the idea of alcoholic poisoning could not be entertained, the patient, who was admitted from the Richmond Lunatic Asylum, presented a scarlet colour of skin, with blackish spots here and there, and one very dark spot at the point of the nose. He was spitting up blood from the lungs. He also died rapidly, and at the *post-mortem* examination, it seemed as if some *vis a tergo* forcibly detached the cuticle from the cutis; the cuticle could be rubbed off in large flakes, leaving the cutis underneath deeply injected with blood. Other instances of this remarkable form of disease occurred. One very remarkable case was that in which the patient had been confined in the Rotunda Hospital, and was sent to us with the disease subsequently. She did not develop the purpuric condition to the maximum intensity; there were a number of spots in various parts of the body, but not in an extreme degree. She, however, died rapidly within two or three days after admission. It would be out of place here to enter into a discussion originating on this singular condition; but I cannot help remarking what an extraordinary resemblance this form of the malady exhibits to those cases of purpuric disease which occurred during the epidemic of cerebro-spinal meningitis. I cannot also help calling to mind that during the great epidemic of yellow fever, at Lisbon, cases were presented to me, which exhibited precisely the same colouration of skin, and the same general appearance as those I now mention. On a future occasion when dealing with the greater question of the probable essential unity of all diseases, I purpose treating of this subject. It makes one reflect what agency it can be that acts on the spinal-cutaneous system, and impels to the surface a quantity of blood in such extraordinary excess as occurs in these cases of apparently very opposite forms of diseases.

Main features of the Disease.—I shall run briefly through the subject of the main features of the disease. Delirium was present in a great number of cases, but as has been well observed on previous nights here, it was by no means a fatal indication. Many cases with marked delirium recovered. No special conditions of the pulse and heart occurred, unless we had these particular forms where the pulse remained at the original standard.

The pulse was very variable, little above the standard in many cases which progressed to a fatal issue; and in the most rapidly fatal purpuric cases it often gave no indication of danger. In a very striking instance of the worst form of the purpuric variety (but not in the record of the 611 cases) the pulse at 10 hours before death was 76, regular, full, not intermittent, and the sensorium was clear.

With regard to the respiratory passages we had to consider the

state of the uvula, palate, pharynx, and larynx. I have already stated that a great number of cases presented themselves in which these parts were affected, to show you what a considerable amount of the deaths was due to this complication. It has not been, however, the main feature inducing mortality. I have already given statistics to show that of 120 deaths not more than 25 could be attributed solely to complications in this region, including the uvula, pharynx, and larynx.

The digestive system did not on the whole present complications of a very remarkable character. Except in the purpuric cases, where hæmorrhages to a considerable extent occurred, we had no great trouble with the condition of the patients' intestines. The renal system did not in our experience exhibit very marked complications. In fact the number of cases in which albumen in the urine was found was extremely small. I cannot pretend to say that the 611 cases were all examined for albuminuria, but I can state with confidence that of a large number examined with this view, albumen was found in a very small proportion. I can state that in 50 examined carefully albumen was found in one only. Of course I do not refer to those instances in which blood was present in the urine in hæmorrhagic cases. With regard to the cutaneous system I have already said sufficient in dealing with the question of purpura, and that peculiar form of desquamation, throwing off or shedding the contents of the pustules and the superjacent epidermis. We had a good deal of trouble from the occurrence of boils. In one instance I opened successively twenty boils in one individual case, and I may state that this tendency to the occurrence of boils forms one of the very few causes which delay for any lengthened period the recovery of patients in hospital.

A point which I think of the highest interest in this remarkable epidemic is, that so far as our cases have gone in the Hardwicke Hospital, though in not a few instances the eye has been affected, not a single case with permanent impairment of vision has occurred. This is a point in which the present epidemic differs from those in which in former years a large proportion were blinded by small-pox. Several cases have had corneitis, but owing to the care and attention of the clinical clerks, and the cleanliness so rigidly enforced, not one case has lost an eye, or has had the sight permanently impaired, out of the 611 cases that in a year have passed through the Hardwicke Hospital.

Now, Sir, that it may not be stated, as it has been loosely by some persons, that this epidemic differs from others, and is not the veritable small-pox—a notion which has got abroad amongst the public, and which I have heard some gentlemen not having much experience in this disease assert—I have had preserved specimens of two cases of confluent small-pox, which show the condition of the skin in a very remarkable manner. They will be of use in future time by showing what the true nature of this malady was in this epidemic.

Treatment.—I do not propose to test our plans of treatment by any attempt at statistical details. Such a discussion would not be attended with any conclusive results, as it is impossible in so complicated a disease to avoid the use of numerous remedial agencies in the same case, all of which more or less tend to the favourable issue of the disease in cases of cure. I believe, however, I may state with some confidence for myself and my colleagues, that we have not as yet found any drug however vaunted as to its therapeutic powers, to be attended with any thing approaching to specific action in promoting recovery.

Small-pox is essentially a disease in which every case must be treated on its own merits. We have mainly employed the following classes of agents, with each of which some recovered and some died. No one drug exhibited in my judgment any marked superiority over another.

On diaphoretics and salines	-	-	about 100 cases.
On chlorates of potash and quinine (mainly)	„	100	„
On sulpho-carbolates (mainly)	-	-	„ 100 „
Carbolic acid was freely sprinkled through the wards at all times.			
Xylol	-	-	in 25 cases.
Turpentine, iron, and other styptics	-	-	in 100 cases of purpuric complication.
Stimulants, medicinal and alcoholic in a large proportion of cases moderately; in cases with asthenic symptoms, very largely			
	-	-	in 200 cases.

Nitrate of silver, chlorate of potash, sulphurous acid, gargles to the throat were used in numerous cases, and with much benefit.

Leeches to the head in delirium, and to the throat in laryngeal complications were frequently employed, and with great effect.

Here I may observe, though I certainly do not wish to raise any subject of controversy at this moment, that I am not myself an advocate of any view as to the septic condition of the system in this disease. I see nothing in the disease to indicate any true septic tendency; quite the contrary. I observe in cases of death that the tissues are thoroughly well preserved, that no tendency to rapid or increased decomposition has been induced, and therefore I am not an advocate in any point of view, nor do I think anything that has been advanced here or elsewhere goes to sustain the idea, that the pathological condition present is one of septicism, or that the mode of treatment of the disease which would offer any promise of advantage would be an antiseptic treatment. So far as I can judge from the results hitherto published, both from my own experience and that of others, I do not see that the so-called antiseptic treatment offers any advantage, when even in the few cases that have been lately mentioned the mortality reached 36 per cent., and when we know that the mortality of the disease taken at large in our hospital cases is but 19 per

cent. I think that a plan of treatment which can only advance in its favour a mortality of 36 per cent. cannot be taken up with advantage, to the exclusion of others. In fact, to use a common expression, I believe small-pox to be a disease in which every case must be treated on its individual merits.

We cannot report very favourably of the treatment of the disease by baths during the acute period. One case placed in a bath died shortly after it was removed therefrom; but it was undoubtedly in a bad condition when placed in the bath. In the convalescent stage we employed baths very abundantly, and with markedly beneficial results. With respect to local applications I have been in the habit of employing a combination, especially to the face and hands, of zinc ointment, with the oxide of lead ointment of the Dublin Pharmacopœia, glycerine, and a little prussic acid. The advantages of this application are—first, that it is cooling and soothing; in the next place, it prevents, to a certain extent, though not in all cases, pitting; it keeps the surface clean, and in my opinion a row of patients at the side of a ward looks less hideous, when whitewashed with my preparation than when coated with carbolic acid and other preparations having a tendency to assume a blackened tint. However, my favourite preparation is not free from this objection in certain instances, for in a few cases I have observed the surface after this application to become blackened. That was clearly due to the sulphur compounds of the body coming to the surface and blackening the lead ointment, which was a constituent of the preparation I have mentioned.

Conduct of the Hospital Staff.—I cannot pass over the opportunity of remarking on the conduct of the staff of the hospital. I never witnessed at any time more devoted attention than that bestowed on the patients in this loathsome disease by the clinical clerks, at great danger to themselves, as evidenced by the fact that two of them were attacked by the disease. Both, I am happy to say, recovered. I have never seen anything to exceed the care, watchfulness, fearlessness, self-sacrifice, self-devotion, and constant attention, day and night, of the gentlemen performing the office of clinical clerks during this epidemic. The same may be said of our chaplains, and of all our resident officers. I would wish to state publicly that, from my own personal observation at various times, frequently at night, constantly during the day, I can bear the highest possible testimony to the attention of the matron and nurses, and their devotion to the sick. I have seen children become so attached to the nurses as to weep when taken away by their own mothers from the institution.

General Inferences.—From the figures that I have placed before you I think we may deduce some general statistical results which would be of use in understanding the present relations of the epidemic and estimating its lethal force and probable extent through our population, its probable

duration and the financial problems which it involves. On the data already furnished with regard to the 611 cases treated within the year in the Hardwicke Hospital, we may conclude that the mortality of 19·47 per cent. is below the average hospital mortality of the disease in this country in past years, that it is also considerably below the average mortality of small-pox as given in such standard works as those of Copland and Gregory, by whom the average mortality is estimated at 22 per cent.

I need not tell you that the mortality of any number of cases of an epidemic, such as small-pox, treated in hospital cannot be taken in comparison with the mortality of the cases of the disease treated outside. We know that a large proportion of cases occur in every epidemic, of the mildest possible form of the disease; and, to use the very graphic words quoted by my friend Dr. Burke, from Dr. Mansell, "many young and old persons put it over them standing." Consequently, those cases taken into account dilute the total mortality, so that we should not be surprised to find the mortality amongst the public at large, as compared with the mortality in hospital, infinitely small. If the mortality be so low as 7 or 8 or 10 per cent. in the cases treated in their own homes amongst the general public, it is to be accounted for by the fact that it is diluted, as it were, by the large number of mild cases which go to swell the whole body of the epidemic.

A very false conclusion would be drawn from a consideration of those cases treated in hospital alone. Of course, as a matter of fact, we all know that the worst cases are those that go to hospital. They are the worst in many points of view, not only on account of the gravity of the disease, but from the fact that in many instances they have been kept at home until the confluent stage is developed to the fullest, in small, crowded, or ill-ventilated rooms with very inadequate nursing, nutriment, and care.

Cost of Maintenance.—Supposing, which is not unlikely, that the mortality will be tolerably uniform for the graver hospital cases between 611 and 1,000, we may then estimate that for every 1,000 cases the actual mortality would be 196 and a fraction over; that every thousand cases would require hospital accommodation to the extent of 17,771 hospital days; and if we calculate the cheapest mode of providing accommodation with the most reasonable staff and establishment charges, and assuming that each hospital day costs 3s. 6d., which I consider a low standard for the treatment of such an expensive disease, we find that every thousand cases would cost at least £3,000. This is pretty nearly an average of £3 per head, and is sufficiently close to the results furnished by the report of the Board of Superintendence of Dublin hospitals. We find that the hospitals under the management of the Board enjoyed in the year ending 31st March, 1871, a total income of £33,061 3s. for which 11,776 patients were treated, being as nearly as possible at the rate of £3 per head.

Probable duration of the Epidemic.—If we now take as a basis for calculation, that about five per cent. of the total population is liable to be attacked by small-pox in times of epidemic visitation, the number of those liable to be attacked in the Dublin district, with a population of about 245,352 in 1871, would be 12,267, and with a mortality of 19 per cent. on those attacked would amount to 2,330 deaths. But in estimating the lighter general as distinguished from the graver hospital mortality, we are probably justified in placing it so low as from 5 to 10 per cent. If we take it to be half the hospital mortality, the total expected deaths may be taken at about 1,200 to 1,500. From a most important and valuable statistical account, placed in my hands by my friend Dr. Burke before this meeting commenced, I find that 994 deaths have been registered from the 4th March, 1871, to the 13th April, 1872, affording a ratio of 3·9 deaths to the 1,000 living; a most important element in a calculation of the possible spread and probable future duration of the epidemic. Unfortunately we have, as yet, no registration of sickness, and can, therefore, only form an approximate estimate of the extent of the disease. Dr. Burke has already called public attention to this, which is one of the most pressing wants of the day, and I trust that when the Sanitary Bill is before the House of Commons next session, our able representative, Sir Dominic Corrigan, will lend his powerful aid to secure a suitable system for the compulsory registration of sickness, as distinguished from deaths, under all proper restrictions of course. I believe that no more valuable addition could be made to any bill, than a power to compel the registration of disease. We shall never know what the condition of any epidemic is, what its actual state at any moment is, we shall never be able to heave our log and see how the ship of public health is going, until we have a complete system of registration of disease as well as registration of deaths. I am quite satisfied that Dr. Burke will impress on the government the importance of this subject, and I am equally sure that our able representative, Sir Dominic Corrigan, will succeed in having a proper system of registration introduced into the sanitary bill we are promised next year. As it is probable that between 8,000 and 10,000 cases of small-pox have occurred in the Dublin district within a year, and that the greater part of the expected mortality has already occurred, we may assume that if the rate of progress of the epidemic be uniform, a few months will exhaust the susceptible individuals, and that we shall arrive at the close of the epidemic in less than six months from the present date, possibly in a still shorter period. Dropping cases will, of course, be found to occur, but in view of the fairly protected condition of the population by vaccination, there are no grounds for the indulgence of any public panic; and it cannot but have a reassuring effect on the community at large, if our statistical inquiries show with any reasonable degree of probability that the epidemic has passed its

culmination and approaches its close. I am fully aware that medical prophecy is a dangerous and tempting kind of intellectual indulgence, but I have reason to think that my estimate will not be found far from the mark. So far the epidemic, in the midst of which we are, has not we find been as widely spread or as fatal as the visitation in the sister island; and so far my conclusions given to the public in a clinical lecture, delivered on the approach of the epidemic in February, 1871, have been realised.

Convalescent Homes.—I may now touch on a question which has recently agitated the public mind a good deal—I mean the establishment of convalescent homes for those recovering from small-pox. I am sorry to differ from many of my friends and colleagues in their views on this subject, and while I believe that nothing could more redound to the charity of our community than to come forward, as they have done, to relieve want in the families of those struck down by the epidemic, I cannot think that the assemblage of numbers of men and women in one or several buildings would be attended with good results, or would meet the requirements of the case. In my own experience I have found the greatest difficulty in keeping patients in hospital whose convalescence has been fairly established. Convalescence from small-pox, it must be remembered, has no similarity to convalescence from ordinary fevers. A patient who is to-day in peril of his life from small-pox, may this day week be up and walking about, and in possession of a large, if not full measure of health and vigour; and I could almost at any time brigade a party of convalescents from the Hardwicke Hospital, ten, twenty, thirty, or more in number, able to eat and drink with, aye, or fight any similar number of their age and weight. The fact is, convalescence from even bad forms of small-pox is often extremely rapid, and the patients are soon again in possession of full strength, and with their appetites and animal instincts remarkably developed. Unless separate homes for males and females were provided, I should much fear the moral results. I believe, then, that convalescent homes are unnecessary, and would not be availed of except by the idle and perhaps dissolute; and a community harder to manage than the inmates of such a home I cannot well conceive. I must on the other hand, however, protect myself from any supposition that I am an advocate for prematurely dismissing patients from hospital, and thus widely disseminating contagion through the community at large. I am an advocate for the erection and extension of convalescent departments in connexion with the larger hospitals, in which alone I conceive small-pox and other zymotic diseases should be treated. Sheds, or tents, or marquees, could be readily provided and at little expense, in which the convalescents from small-pox could be placed and retained, so long as it was found possible to induce them to remain. It must be remembered that there is no power known to the law, by which any man reasonably

convalescent from small-pox can be detained in any hospital, or any convalescent home against his will.

Duration of Convalescence.—What may be the period within which a convalescent from small-pox is capable of communicating the disease to another, it is not easy to define. I would not myself place it at less than six weeks, and to be efficient as a protective agency, confinement in a convalescent shed or home should be rigidly enforced for this whole period. The illegality and impossibility of such a forced imprisonment for so protracted a period, to say nothing of its cost to the public, at once puts such a proposition out of court altogether. Anything short of this would be ineffectual; to carry it out would be both illegal and impossible.

Baths.—There is one sanitary agency which I believe it will be readily in the power of the public authorities and the charitably disposed to place at the command of the poorer members of the community—I mean the establishment, on a large scale, and in numerous localities, in the poorer districts of the city, of a cheap and inexpensive system of baths. If there is any one agent more than another in which I believe as a disinfectant capable of being used sufficiently extensively for the purpose by the poorer members of the community, it is soap and water abundantly applied to their persons and their clothing. Cheap baths could be readily established, in say fifty centres throughout the city and put in operation within a week. To these, I have no doubt, the poor would readily resort, and free ablution is in my mind the speediest and most effective of all antiseptics. Sulphides, chlorides, carbolic acid, and other disinfectants may also be employed, and without large additional cost to the public.

Vaccination.—In conclusion, I have only one further suggestion to make, and that is to enforce again what all our papers here have conclusively shown and what I fully believe in—the protecting power of vaccination. If I had but one word to say of the epidemic by way of advice it would be “vaccinate, vaccinate, re-vaccinate.”

The history of this epidemic will be a very interesting one indeed. I would throw out then as a suggestion that it might be well to appoint a committee of some of the members of this Association, who, in conjunction with the dispensary medical officers of Dublin, and aided by the guidance and direction of Dr. Burke, might present us with a most accurate physical, medical, pathological, and statistical history of this very remarkable epidemic, which has come upon us at a time when we thought we were almost free from this destructive and loathsome disease.

I have very profoundly to thank you, sir, and the Association for the great patience with which you have heard these dry details, and, I fear, tiresome statistics. I have done my humble part, very imperfectly, I am aware, to contribute towards the history of the epidemic so far as it has been observed in the Hardwicke Hospital during the past year.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF
DUBLIN.

Amyloid Disease of the Liver.—DR. WILLIAM MOORE said that the specimens he now brought under the notice of the Society were taken from the body of a man whom he first saw in June last, in the Royal Hospital. At that date the patient had a cachectic appearance; and, on making an examination, his liver was perceptible beneath the floating ribs, and some tubercles could be felt in it. He was also slightly ascitic. About a fortnight ago, he received a letter from the Surgeon-Major of the Royal Hospital, asking him if he would take the man into Sir Patrick Dun's Hospital. Accordingly, he was admitted on the 23rd of October. He was a soldier, 38 years of age, and had served in the Ionian Islands, Malta, the Cape of Good Hope, and Gibraltar. He did not acknowledge to be a drunkard; but admitted to having had syphilis twice severely. He said he had had a fever at Gibraltar, and since then he had never felt well. A year ago he first complained of pain in the epigastrium, and that was shortly followed by a cough, and he also felt that he was getting swollen. What he complained most of was this troublesome cough with expectoration. About a year ago he vomited blood on two occasions.

His appearance on admission to Sir Patrick Dun's Hospital was as follows:—He was emaciated to the greatest possible degree, and had a peculiar red scaly patch on his forehead. He was not jaundiced, but was of that dusky colour commonly associated with malignant disease. He had a very troublesome cough; his expectoration was purulent and frothy, with hæmoptysis, which was scanty but continuous; it was not of a profuse kind or of a venous character, being more of a bright currant-jelly-like appearance, nor was the expectoration very profuse. There was no deformity as regards the clavicle; the left side was perceptibly smaller than the right—still, there was no twisting of the ribs—there was not that deformity associated with chronic pleurisy. The right side of the chest was normal in appearance. When the chest was percussed, the superior portion of the left side was comparatively clear in front; but the scapular region posteriorly was dull, as was also the mammary region in front. On the right side the chest was perfectly clear, down to the mammary region, and there dulness was perceptible. There was gargouillement over the whole of the left side, both anteriorly and posteriorly. Over the left lung respiration was tubular, and inferiorly accompanied with gargouillement, whilst the respiration in the right lung was puerile. The ascites was very great. The measurement, shortly before the man's death, was 40 inches round

the umbilicus, and the epigastric veins stood out in a marked manner. The urine was examined several times while the man was in hospital, but there was no trace of albumen, and its reaction and specific gravity were normal. He rejected everything, and complained of epigastric pain of a very severe character. It should have been mentioned that, when first admitted to hospital, there was a visible tumour in the epigastric region; but this gradually disappeared as the fluid increased. There was dulness on percussion over the right mammary region, the right hypochondriac, lumbar and epigastric regions, and there was visible displacement of the heart upwards and forwards. He had diarrhoea some days before death; but this was checked, and he sunk from complete exhaustion. Dr. Moore did not mean to say there was anything wonderful in the progress of this case; but it was a type of a class of cases often met with, and which were calculated to mislead one as to their specific character. The man was not jaundiced, but had all the appearance of malignant disease.

On making a *post-mortem* examination, the liver was found to be nearly double its ordinary weight; it weighed 8 lbs., and its transverse diameter was 13 inches. There were numerous nodules, some of which measured transversely $3\frac{1}{2}$ inches. The edges of the liver were thickened and rounded, and it was an excellent specimen of amyloid or lardaceous liver. The vascular structure of it was almost obliterated; and, on making a section of the tubercles, and testing them in the usual manner, with iodine, they presented an amyloid character. The kidneys were not altered in size, but presented the same appearance on being tested with iodine. The spleen was amyloid and slightly enlarged. The heart had all the appearance of a heart in a state of fatty degeneration. The left lung was universally adherent and diminished in size by one-half; the pleura universally thickened. On making a section of it, numerous cavities were found, varying in size from a walnut to a pea, and the bronchial tubes dilated. What remained of the pulmonary structure was as hard as fibro-cartilage, and of a greyish white colour, more especially the lower half of the viscus. The right lung was rather emphysematous, and presented some tubercular deposits at the apex. Dr. Moore regarded this as a very good specimen of cirrhosis, and it reminded him of a plate of this disease described by Sir Dominic Corrigan in a former volume of the *Dublin Quarterly Journal*.

As regards the liver, its deepled fissured, nodulated surface, with bands of connective tissue passing through the viscus, its pale yellowish colour, and hard, brawny consistence, with, in some places, thickened capsule, would lead him to call it a case of the large lardaceous or amyloid liver, with "cirrhotic" induration, as described by Frerichs.—*November 25, 1871.*

Dissecting Aneurism of the Aorta.—DR. HAYDEN said: I wish, Sir, to submit to the Society an example of fatty disease of the heart, and aneurism of the aorta, which proved fatal by rupture into the pericardium. The case has already been made the subject of public notice in this city, and an inquest has been held on the body of the deceased, and, therefore, although a private case, there can be no breach of professional decorum in bringing it under the notice of this Society; but there is a positive reason for so doing, in the great interest the case possesses as an example of latent aneurism, and latent fatty degeneration of the heart. Three years and a-half ago this gentleman consulted me for the first time. He was a professional man of great ability, aged sixty-two. His symptoms were those of rheumatism of the lumbar region, extending to the diaphragm, with more or less of diaphragmatic pleuritis. He was an exceedingly nervous man and disposed to exaggerate all his symptoms. I understood that about the same time he also consulted Dr. Stokes. He recovered from this attack after a protracted illness, and was able to resume his professional duties, except that he was never able, as he told me, to undertake heavy professional work. He was able, however, to travel to distant parts of the country. He was very fond of pedestrian exercise, and I have repeatedly seen him walking in the streets, always at a slow pace, but apparently well. A year later he visited me again, and complained of neuralgia of the face and arms. There was nothing to attract special notice in his condition. I treated him simply for neuralgia, and he seemed to have recovered his usual health. On the 10th of this month he visited me for the last time, with the view of obtaining relief from neuralgia of the most aggravated character from which he suffered. He described the pains as shooting over the face, scalp, neck, down the right arm, and round the right side of the chest; and pointed to one spot midway between the right nipple and the sternum, where, as he declared, the pain was fixed. His general condition was apparently satisfactory; pulse 84, regular, and equal on both sides, but rather weak; appetite good, and all the functions regularly and effectively performed. The pain usually ceased at night and his sleep was good and unbroken. On this occasion I made an examination of his chest, confined, however, to the precordium and the site of the alleged pain to the right of the sternum. The action of the heart was quite regular; the first sound, however, was ill-pronounced and feeble; the second sound, both in the precordium and in a still greater degree at the site of the alleged pain, near the right margin of the sternum, and on a level with the nipple, was remarkably short, sharp, and of a ringing quality. The pupils were normal; he was not subject to vertigo, fainting fits, or any general derangement of health, and had never spat blood. There was no *arcus senilis* or oedema. He stated that he had a harsh, dry cough, which, however, I had not the

advantage of hearing. I made little of his case, in this sense, that I was not disposed to attach much importance to his symptoms, and was about to prescribe for his neuralgia, and turned round to my writing-desk for that purpose. He was a very intellectual man, and both of us being interested in a particular matter, I resumed the conversation for a moment by asking him a question on that topic; he answered the question rather excitedly, as was his habit. I resumed my position at the desk, the patient being at my right hand side. I heard a rustling movement; turning round I saw him fall off the chair towards his left side and upon his face. It occurred to me that the man had been subject to epileptic attacks, which he had concealed from me. I lifted him up and at once saw the matter was more serious. His neck became turgid; the pulse ceased; he made one or two gasping respiratory efforts. I turned him over on his back, having hastily summoned the servant from the adjoining hall; a shadow passed over his face from below upwards,—a veritable “shadow of death,” manifestly the result of reflux capillary venous engorgement, and he was dead. The whole period occupied by the occurrence just described did not exceed three minutes.

As there should be a complete examination of the body, I notified the matter to the coroner, and with the help of my friend Dr. Collins a careful examination of the chest was made two days subsequently to death. The chest alone was examined. The anterior mediastinum was found laden with large masses of yellow fat. The pericardium was distended with serum and dark clotted blood, fully to the amount of $1\frac{1}{2}$ to 2 lbs. The surface of the heart was quite yellow; the heart itself was somewhat enlarged; it was remarkably soft and flabby, expanding by its own weight when laid upon the table. The root of the aorta exhibited a rent on its right aspect, half an inch above the valves, by which the blood escaped from the vessel into the pericardium. The aorta was greatly dilated immediately above the orifice, and the internal and middle coats, the latter of which was highly atheromatous, had given way and the blood had insinuated itself between the external and the middle coat of the vessel, and formed a dissecting aneurism, which, by rupture of the external coat into the pericardium, was the immediate cause of death. The aorta gradually expanded from the sinuses of Valsalva upwards, into an aneurism of the size and shape of a pineapple, and capable of containing at least twelve ounces of blood, and engaging the entire circumference of the vessel as far as the descending portion of the arch. The internal surface of the sac was rugose, highly atheromatous, and in several places fissured and rent. The three primary branches were strictly normal, as was likewise the aorta itself, in regard to diameter, beyond the transverse portion of the arch; the œsophagus and the trachea had escaped pressure, and hence there was no dysphagia or dyspnœa. The apertures of the three great vessels arising from the

arch were of normal size, and hence there was no difference in the radial pulse on the two sides; the left ventricle was dilated and thickened. This was evidently the result of the atheromatous condition of the aorta, and at least the outer half of its wall consisted exclusively of fat. A section of the outer fatty portion of the left ventricle, steeped in sulphuric ether for 24 hours, and examined under the microscope, exhibited fatty accumulation and substitution, as distinguished from fatty degeneration; fat had accumulated on the surface of the heart, and grown inwards, displacing and causing atrophy of the muscular substance, detached fragments of which, reduced in size and altered in appearance, were visible amongst the fat cells. The internal portion of the ventricular wall presented the normal appearance to the naked eye, and its fibres exhibited transverse striation. The fat cells were large and partially emptied of their oily contents by the action of the ether. The right ventricle was in a more advanced stage of similar transformation; the entire thickness of the wall, with the exception of a layer on the internal surface, not thicker than a threepenny piece, consisted of fat, and presented on section the appearance of suet. Microscopically examined, the structure was found identical with that of the left ventricle, but in a more advanced stage of metamorphosis. The right auricle was laden with fat, and its walls consisted almost exclusively of it. This case is of interest in regard to the latency of a two-fold disease of a most formidable character; because, with the exception of the harsh dry cough, which I never heard, and the localized neuralgic pain, there was not one symptom to direct attention to the chest; but the pain, being associated with general neuralgia, was of no special significance. The dull character of the first sound, and the sharp, ringing quality of the second, excited a passing suspicion of weak heart and aortic atheroma; but in the absence of evidence more definite, did not seem to warrant a suspicion of formidable disease. Complete and careful examination of the chest was therefore not made.—*November 25, 1871.*

Disease of the Knee-Joint.—MR. J. HAMILTON exhibited a specimen showing the effects of long-continued disease of the knee-joint. A young man, aged 20, was admitted into hospital on Wednesday last, suffering from the most excruciating pain in the knee-joint; it was incessant, and occasionally accompanied by painful starts, which were so severe that they made him almost scream out. The joint was exceedingly swollen; the swelling was of an oval character, fluctuating and white on the surface; the slightest motion, gave him great pain, and he shrank from the least attempt to touch or move the limb. It was clear from the local symptoms that there was suppuration within the joint, that there was ulceration of the cartilages, and that so far as the joint was concerned it was a case in which cure could not be looked for or

even amendment. The constitutional symptoms were exceedingly severe, and pointed to the necessity of acting at once. He was emaciated to the last degree, had profuse sweats, was delirious, sometimes in the day and always at night; his pulse so rapid that it could not be counted, and very weak. It was decided as the last hope at once to amputate the limb, and he was brought from his lodgings to the hospital for that purpose. The history of the case showed it to be one of those cases in which the disease first commenced in the synovial membrane and then went on to other structures of the joint. It began about six years previously, when the patient was 14 years of age, with pain in the joint and swelling, and sometimes inability to keep the limb straight. He underwent treatment several times, and always with advantage, but never got completely well, and was constantly subject to relapses. At last, about a-year ago, he began to suffer from symptoms clearly indicating the presence of ulceration of the cartilages of the joint, from violent and painful starts at night, and inability to put his foot to the ground, the slightest touch of the heel or sole giving him exquisite torture. The limb was put into a fixed position, and he got better for a time; but unfortunately he fell down stairs and broke the femur about four inches above the joint. By keeping him quiet and putting the limb into leather splints, he got a good deal of relief; but an abscess then formed in the back of the joint, and on opening this the matter was discharged, and he left hospital with the splints on, and could get on tolerably well. He (Mr. Hamilton) then lost sight of him from the latter end of May until he came into hospital on the Wednesday previous, when the symptoms were so urgent that it was clear there was no time to be lost in amputating the limb, which was accordingly done. Mr. Hamilton then, in the presence of the Society, opened the joint, from which an enormous quantity of pus, tinged with blood, flowed out. He called attention to the ulceration of the cartilage of the patella, to the extent nearly of a florin, and to the existence of little fibrous bands between the articulating surfaces, being an attempt at fibrous achylosis. The surfaces of the condyles of the femur were denuded of cartilage, and the same was the case with the external condyle of the head of the tibia; the internal condyle was also ulcerated, but to a less extent. The femur had been broken about six inches above its lower extremity. The fracture was very oblique and had become firmly united by copious bony deposit between the broken surfaces. The synovial membranes of the joint was thickened, pulpy, and of a livid red colour. The bursa above the knee communicated with the joint, and was similarly diseased, its membrane thick and red, and it was full of pus.—*November 25, 1871.*

Aneurism of the Thoracic Aorta.—DR. MACSWINEY said the morbid specimens which he exhibited were removed from the body of a woman,

aged about 40, who died some five or six weeks previously in Jervis-street Hospital. At the time of their removal the parts afforded a good illustration of the pathological condition present in a case of aneurism of the arch of the aorta, where death took place without actual rupture of the sac. They were now considerably altered in appearance—but sufficient evidence of the more remarkable morbid changes remained to justify their presentation to the Society. The history of the case, so far as he knew it, was as follows:—About two years ago the woman presented herself as an out-patient at the dispensary attached to Jervis-street Hospital. She had considerable œdematous swelling of the right side of the face and neck. She complained of pain in her chest, extending from the apex of the heart to the top of the right shoulder. She suffered from palpitation of the heart, dyspnœa, and cough. Her face had a livid appearance. His colleague, Dr. Martin, received her as an intern patient into the Infirmary. At this time she presented the following “signs:”—There was a slight prominence at the upper portion of the right side of the thorax in front; it was situated at the position of the second rib and the intercostal space above and below it. No movement was to be seen, nor was there any vibratory thrill to be felt by the hand placed upon this prominence. Percussion over the space corresponding with the prominence caused pain, and revealed the existence of dulness there. The veins of the neck at the right side were abnormally large and full, and the superficial veins of the right side of the thorax were unusually prominent and enlarged. Auscultation detected two centres of sound in the chest—one at the normal situation of the heart’s beat and sound, the other audible over the situation of the prominence at the right side of the chest. The heart sounds were healthy. The sound that was heard over the prominence was a double one, louder than the heart sounds, and unaccompanied by a bruit. The right lung, posteriorly, was found to be affected with bronchitis. There were mucous sibilant, and sonorous râles audible there upon auscultation. The “symptoms” present in this poor woman’s case were these:—Her face was livid; there was œdema of it, of the neck, and part of the chest. She had occasional dysphagia. She suffered from paroxysms of dyspnœa, which sometimes heightened into orthopnœa of most extreme and distressing severity, occasionally threatening dissolution. She had cough of a distressing kind, harsh and ringing in sound, and attacking her principally, or with increased severity, at night. She suffered much from pain, which she described clearly as of a double or two-fold character, viz., there was, first, a constant, dull, distressing, aching pain referred to the chest; and, second, a paroxysmal, intermittent, lancinating pain, extending through right shoulder, arm, forearm, and to tips of fingers, extremely severe when at its height, and then producing, temporarily, a condition of partial paralysis of the hand.

Her pulse did not exhibit any very remarkable character; it was somewhat feebler at the right side than at the left; the right pulse, also, was distinctly after the left pulse in time; its systole was after the systole of the left side. Her pupils were quite regular. At no time while she was under observation was there any abnormal dilatation or contraction of the pupils noticed. Sometimes we perceived that her hearing and sight seemed to be impaired. This occurred but seldom, and always corresponded with periods of great suffering from pain and orthopnoea. This patient left and was re-admitted into hospital several times during a period of over two years, and was about alternately under his (Dr. MacSwiney's) and under Dr. Martin's care. Ultimately, she died in the hospital under his (Dr. MacSwiney's) care. The "diagnosis" of aneurism was made in this case at the time of her first admission, and he (Dr. MacSwiney) had himself presented her to Drs. Hayden and Gordon—the Examiners from the College of Physicians, upon the occasion of their holding a medical clinical examination in the hospital last summer, as a well-marked example of a rather rare disease—aneurism of the arch of the aorta in the female. During the poor woman's last illness, she suffered principally, and very much, from distressing cough and orthopnoea. Her face was purple, her cough almost incessant, the muco-purulent matter expectorated enormous in quantity, and she was wholly unable to lie down or go to sleep. She knew she was dying, and told us that it was just going to break with every cough. And yet, as the "*post-mortem*," now to be described, showed, she died without the aneurism having ruptured, and of intense tracheo-bronchitis, induced by the pressure of the tumour on the wind-pipe.

At the autopsy, made 24 hours after death, the following appearances and condition of parts were noted:—The right lung was generally and strongly adherent, the pleural cavity being almost entirely obliterated. The lungs were both congested. The posterior inferior portion of each lung was consolidated. The anterior upper part of the left lung was emphysematous. The bronchial tubes were dilated extensively, and intensely inflamed, and contained a considerable quantity of a muco-purulent fluid. The pericardium was much thickened, and showed evidence of an antecedent inflammation. It was pushed downwards by a large tumour, which was closely incorporated with it, and prevented its superior part from being accurately defined. The heart was, if any thing, under the average size; its surface was rough and adherent, at parts, to the pericardium. The surface markings of the heart were very indistinct, its cavities displaced, and its general position changed, being pushed downwards, and to the left. Right auricle small, and pushed backward by the tumour. Its anterior and right wall was closely adherent to the mass consisting of the pericardium and lower wall of tumour. The opening of the superior cava was not to be distinguished; it was, probably, represented by a

small vessel which opened into the auricle, and will be again alluded to. That of the inferior cava was abnormally small. The auriculo-ventricular opening admitted four fingers. The right ventricle was rather small. The cusps of its valves were slightly atrophied, but otherwise healthy. The pulmonary opening was small and contracted. The left auricle was normal in size, but part of its posterior wall was adherent to the tumour. The pulmonary veins were below the natural size on both sides. The mitral opening was normal. The left ventricle was healthy. The aortic opening contracted. The sigmoid valves perfect. The superior vena cava, the right innominate, part of the left, the right subclavian vein, and the lower part of the internal jugular, were all completely obliterated. The external jugular vein was much dilated, and joined a very large supra-scapular vein. The trunk formed by these veins ran for some distance in front of a cord which represented the right innominate and the upper part of the cava. This cord was lost in the wall of the tumour, and the vein opened into the right auricle, as previously intimated, being now about one-third of an inch in diameter. The continuation of the left innominate could not be traced beyond the middle of its course. It was much contracted, as were also its tributaries. The aorta beyond the tumour was below the natural calibre, as were also the brachio-cephalic branches. The aneurism extended from within half an inch of the aortic opening to a little beyond the origin of the left subclavian. The tumour might be looked upon as consisting of two aneurisms, or as being a bi-lobed aneurism, the larger lobe being near the heart. This was about five inches long by four broad, and contained a medium-sized clot. Its walls were of unequal thickness; at the lower part they were about three quarter inch, and at the upper and back part about one-tenth inch thick. It projected from the right and posterior side of the artery. The smaller tumour was situated on the transverse part of the arch, and included the origins of the brachio-cephalic vessels. It was spheroidal, and above one and a quarter inch in diameter. It contained a clot which nearly filled it, and yet did not interfere with the orifices of the branches, as the aneurism was from the back of the vessel where it lies in front of the trachea, to which it was closely adherent. On examining the internal surface of the trachea, two spots were seen on the part opposite to where the aneurism pressed externally; they were round, red, partially ulcerated through, and here the aneurism would have burst had not death from bronchitis occurred.—*December 2, 1871.*

Mollusum Contagiosum.—DR. WALTER SMITH exhibited a drawing of a form of disease which, though sufficiently common in its minor manifestations, was yet rare, in an extreme degree, in the form and to the extent delineated in the portrait now before the Society. The drawing, which was executed by Mr. Burnside, represented a much more

pronounced example of the disease than that delineated in the plate published by the Sydenham Society. It was a delineation of the disease called *molluscum sebaceum*, as presented by a man who was admitted into the Adelaide Hospital suffering from bronchitis. On examining the patient in bed these appearances were discovered. The points to which he wished to call attention were—the number of the tumours, their size, and their absence from places where they were usually found. Their number amounted to many scores. They were found on most parts of the body, on the neck, chest, arms, and a few on the legs. The largest was over the left rectus abdominis muscle, and was three and a quarter inches by three inches in size. The next largest was in the left groin, and was two and a half inches by one inch and a half. One of the largest of these tumours on record was that mentioned by Mr. Erasmus Wilson, and it was only three and a half inches in diameter.

These tumours were sessile, mostly fixed on a broad base, and were absent from the genitals and face, situations in which they were usually found.—*December 2, 1871.*

Fracture of the Atlas.—DR. E. HAMILTON said he was indebted to his colleague Mr. Colles for the privilege of laying before the Society an example of fracture of the spine which presented some features of interest. The specimen which he exhibited consisted of the cervical vertebræ of a man forty years of age, who had fallen from a height of some sixteen or seventeen feet. As far as could be learned he fell upon his vertex, for there was no mark of injury anywhere else. There was a contusion on the top of the scalp, showing the great probability that he had fallen head foremost, and that his vertex had struck the ground. He was seen in the act of falling, and was therefore very soon after the accident attended by Mr. Colles, the resident surgeon at Steevens' Hospital, who was sent for to see him at the concern where the accident happened. When seen by Mr. Colles he presented the appearance of collapse, which he (Dr. Hamilton) thought was peculiar, having regard to the injury. It was collapse characterized more by failure of the circulation than by what they should rather have expected to find—failure of innervation; for he rallied in a short time, recovering consciousness in a very few minutes after the accident, and was able to give an accurate and detailed account of what had occurred to him. This was a most important matter, to which he wished to direct the particular attention of the Society. The man was removed to hospital and submitted to a careful examination. From a line an inch above the nipple down to the extremities there was complete loss of muscular power, both voluntary and reflex. On irritating the soles of the feet there was to be seen a slight flickering movement of the toes, but he attributed this rather to direct excitation than to reflex

movement. A similar condition had been noted in other recorded cases. There was also a complete loss of sensation from the nipple down, and they could draw a line across, which was the border line of sensation and anæsthesia. There was priapism. The surface was cold and clammy; the temperature was low, and the respiration was carried on by the diaphragm, the intercostal muscles being thrown altogether out of action. The pulse was below 40. On examining the neck they found the man's head thrown very much backwards, almost reminding one of opisthotonos. On passing the hand lower down detected upon a prominent and tender spot, pressure on which caused pain; and from the appearances presented they hazarded the diagnosis, that there was a fracture or injury producing pressure on the chord at about the fifth cervical vertebra. He had complete power of the upper extremities; he had power of deglutition, and considerable motor power above the point indicated by the line. On the next day he presented somewhat similar symptoms. He rallied a little; his pulse rose in frequency, and the temperature was also a little raised. There was retention of urine; the priapism was continuous and even more marked, and he complained of a remarkable symptom, which he (Dr. Hamilton) could not satisfactorily explain, that at the line of junction between sensation and insensibility there was hyperæsthesia. He complained of extreme tenderness, of a feeling as if the part were burned if anything touched it, just at the border line. He sank, became weaker and weaker, and at last died apparently of asthenia, having survived the accident forty-eight hours. The temperature ranged from 85 to 98. In making the *post-mortem* examination the first thing that struck Mr. Colles on passing in his finger to detach the spine was that the atlas was perfectly comminuted: a fracture traversed the anterior arch. The posterior arch was broken in two places; the right transverse process was broken, but the odontoid process was uninjured. Further down they found a fracture, with displacement of the sixth cervical vertebra. They found it thrown backwards, and the intervertebral substance torn across; the laminæ of the vertebræ were torn, and hence there was great pressure on the spinal chord. The peculiarity in the case was, in the first place and especially, the amount of injury done to the atlas, while the cranium was perfectly uninjured. It seemed as if the entire force was carried from the vertex to the atlas. It was most remarkable, also, that with this great amount of violence there should have been such a slight effect on the brain. The concussion of the brain passed off very quickly, which was greatly to be wondered at under the circumstances. The other symptoms were such as might have been expected from a fracture occurring in the neighbourhood of the sixth cervical vertebra.—*December 16, 1871.*

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PART I.

ORIGINAL COMMUNICATIONS.

ART. XV.—*Notes on Erythema Nodosum.* By H. S. PURDON, M.D., Physician and Lecturer on Clinical Medicine, Belfast General Hospital; Physician to the Hospital for Diseases of the Skin; Corresponding Member, New York Dermatological Society, &c.

ERYTHEMA NODOSUM is so well known to the profession that it may seem presumptuous to offer any remarks upon the subject, especially as I have nothing very new or original to suggest; however it is a disease that is common amongst young females who are employed in warehouses and factories, and as I have had frequent opportunities of observing this complaint at the Belfast Hospital for Diseases of the Skin, there are two points to which I think attention may be directed—1st, as regards its forming one of the varieties of erythema, and 2nd, as regards its treatment.

Mr. Hutchinson, a no mean authority on dermatological matters, looks on erythema nodosum as an “abortive exanthem,” but I, for my part, believe that its classification with hæmorrhages would be more correct. Now, hæmorrhagic affections of the skin are peculiar to the cutis, for although the blood be effused among the epidermic cells, still hæmorrhages cannot, originally at least, constitute an epidermic affection, whilst erythema is a superficial inflammation of

the skin, not affecting the subcutaneous tissue, except in that variety which is the commencement of one of the forms of eczema. In erythema nodosum, which is ushered in by febrile symptoms, neuralgic pains in the limbs, &c., &c., the protuberance, most frequently seen parallel to the tibia, are due to hæmorrhage, the centre of the oval node-like patch being filled with blood. Dr. Bohn regards erythema nodosum as the same affection as peliosis rheumatica, each protuberance representing an inflammatory infiltration arising from embolism of the cutaneous capillaries. Moreover, there is in the first instance œdema of a cluster of capillary loops. The subsequent extravasation of blood may occur in either of two ways, viz., either from rupture of the vessels, or from escape of blood corpuscles, without rupture. The latter is the most recent theory regarding certain forms of extravasation of blood into the tissue of the cutis, as held by German observers. The peculiar shades of colour seen in the patch of erythema, from faint red at first to green, &c., are due to this effused blood, and Nevman thinks that the transformation of hæmatosine of the extravasated blood, which either remains in the blood corpuscles as minute aggregations, producing reddish-brown maculæ on the skin, or hæmatoidin crystals are formed, which produces varied pigmentation.

The lower extremities are not always the seat of erythema nodosum—the arms, for instance, are occasionally attacked. Probably vaso-motor nerve derangement is an important factor in causing this disease, which usually appears in delicate, pale, thin, and anæmic young girls, often accompanied by chlorosis. Now, all causes of debility tend to perverted innervation. Moreover, the disease under notice (erythema nodosum) has been observed associated with chorea.

In addition to the hæmorrhage already mentioned, Nevman (Pullar's translation of his work) believes that in erythema nodosum there is—first, œdema of the superficial layers of the cutis, the eruption being then colourless. The disease is sometimes chronic, and, according to the same author, in cases where the œdema occurs on the extensor surface of the knee-joint it may be confounded with serous effusion into the joint, especially when the œdema fills up the hollows on either side of the patella. To occasion œdema, M. Ranvier (*the Lancet*) states that the vaso-motor nerves of the part must be paralysed. He also found that the mere ligaturing of a vein was not sufficient in a dog to give rise to œdema; but it was caused by section of the vaso-motor nerves.

To come to the treatment. Dr. Spender, of Bath, in an interesting article on cutaneous diseases of the lower limbs (*Journal of Cutaneous Medicine*, Vol. iv.), remarks that although erythema nodosum comes to an end spontaneously, and leaves behind no ill effects, still the constitutional affinities of the disease indicate what may and ought to be done. He prescribes sulphate of magnesia with sulphate of iron and dilute sulphuric acid, and recommends sea bathing or shampooing of legs with soap and hot water, as also a Domette flannel bandage, as nearly all erythematous legs are below the normal temperature. Pure neurotic remedies, Dr. Spender thinks, like quinine and strychnine, do little good by themselves ; but they may assist the hæmatic power of iron, and arsenic may favourably influence the vaso-motor nerves. In addition to the above excellent remarks, I have merely to add that, as a rule, I find no difficulty in "curing" erythema nodosum by prescribing the compound iron mixture and compound decoction of aloes combined, and adding, in cases where the catamenia are suppressed or scanty, to the mixture borax, whilst locally, if pain is much complained of, a lotion of acetate of lead and opium or iodoform ointment (one drachm of iodoform to an ounce of lard, a few drops of rectified spirit being used to dissolve the iodoform) which is a good anæsthetic, are used. A flannel bandage being applied every morning and worn during the day, however, especially in people more advanced in years than those to which my remarks refer, and in whom this disease is not only rare, but also very obstinate, the per-oxide of hydrogen and tincture of the perchloride of iron are the remedies to be recommended ; the dose is a teaspoonful of the per-oxide and ten to twenty drops of the tincture of iron in a wineglassful of water thrice daily. (It will not do to order these two medicines together as in a mixture.) Dr. Brown-Sequard has recorded the fact that oxygen and strychnine contained in the blood have the power of acting on the excitability of nerve-fibre in the nervous centres or in nerve trunks or filaments, and thus the per-oxide, I should say in erythema nodosum tends materially toward restoring a healthy tone in the part affected.

I have said nothing regarding the premonitory symptoms of fever with which the eruption is ushered in. If they should be at all severe rest in bed and an effervescing saline aperient is all I believe that is necessary. In conclusion, there is one complication of erythema nodosum, which is very rare, and that is denied by nearly all dermatologists, Hebra amongst them, viz., ulceration. I have recorded in the editorial commentary, Vol. iv. of *Journal of*

Cutaneous Medicine when under my management, a case of a female mill-worker, in whom there was no trace of syphilis, and in which case several of the protuberances suppurred, leaving painful ulcers. The only author I believe who agrees with me is Hardy, of Saint Louis Hospital, Paris, and who states that if the patient is of a scrofulous constitution, the erythematous protuberances may ulcerate, producing sores like syphilitic ones.

ART. XVI.—*Epileptic Puerperal Convulsions and Puerperal Mania, treated by Chloral Hydrate.* By F. VICTOR M'DOWELL, Ballickmoyler, Queen's County, Member Surgical Society of Ireland; Ex-Demonstrator of Anatomy, Dublin School of Medicine and Surgery.

ON the 1st March, 1871, at 12 p.m., I was called to see a poor woman in labour, named Ellen B., aged 40, living in this district. I was told she had been in "fits" all the day. She is the mother of seven children; the last was still-born, and her health has been very delicate since. For the past month she complained of giddiness and noises in the head, and puffing of the face. On my arrival I found her suffering from epileptic convulsions. The paroxysms occurred periodically, like labour pains, during one of which, and shortly after I had seen her, she gave birth to a still-born child. The placenta was soon expelled without any assistance, and with very little loss.

The patient was totally unconscious, and breathing very heavily; however, the convulsions did not end here, but after a few hours became more frequent.

At the approach of each paroxysm the pulse would become extremely quick and weak, the pupils dilate, the skin turn dark or purple, and the muscles become extremely rigid from head to foot, but would soon be thrown into violent convulsions, during which the distortion of countenance was beyond conception. There was frothing and hissing from the mouth, which was drawn chiefly to the right side, her urine and fœces passing involuntarily. Her head was shaved with much difficulty, four leeches applied to each temple, an opiate enemata administered, and cloths soaked in iced water to be kept constantly on the head.

The "fits" which, previous to this treatment, occurred every

hour, and each lasting about ten minutes, were completely checked, and the woman left sleeping tranquilly.

Heard next morning she awoke *quite well*, but after three or four hours became light-headed.

Saw her next day, and she was then totally unmanageable; the delirium and jactitations were incessant, restraint being sometimes necessary. The patient was now placed in a darkened room, with a careful attendant, the cold application to be kept to her head, and a large blister applied to the nape of the neck. A full dose of calomel and jalap administered, and after its operation two grains of James' powder and half a grain of opium, to be given every three hours for six doses. For five days there was no abatement of the maniacal symptoms, and as the patient had not slept any during that time I determined, as a *dernier resort*, to give chloral hydrate a trial, as I had never heard of its use in such a case before. Twenty-five grains were administered in syrup, and repeated after three hours. This seemed to act as a charm, for the woman had a refreshing sleep of three hours; and a repetition of this treatment for a few nights effected a complete recovery, as a proof of which I may mention I received a visiting ticket on the 5th of March this year, to see her again in labour, and as I found the os well dilated I applied a long forceps, and delivered her of a fine living child, and both are doing well.

It will be seen the above was a remarkable case, and those experienced in dispensary practice can well realize the difficulties of treating such—the convulsions occurring during and after parturition, and terminating in mania.

There was no anasarca. I regret failing to test the urine, and have no further proof of albuminuria than the puffing under the eyelids. The previous history of this patient failed to show that she had ever been attacked by epilepsy.

Much difference of opinion exists as to the action of opium in Puerperal Convulsions; however, I found it act most favourably in this case, and would certainly give it a trial again, provided there was no tendency to apoplexy.

It proved useless in the maniacal affection.

ART. XVII.—*Hæmoptysis*. By J. W. W. NASON, M.D., Univ. Dub.; late Resident Clinical Assistant, Hospital for Consumption, Brompton.

HÆMOPTYSIS, signifying hæmorrhage in general from the lungs, is an incident fraught with so much vital importance as generally, and frequently justly, to excite the gravest feelings of apprehension, not only in the patients themselves, but also among their several friends and attendants. It is in the popular mind almost invariably associated with consumption of the lungs, and not unnaturally, since it is one of the most common complications of phthisis, few cases, comparatively speaking, of pulmonary consumption being wholly free from it; and it is often the first symptom occurring prior or during the course of that disease which, awakening people as it were from a lethargy of listless apathy, brings the hitherto latent disease under their notice. Advice is at once sought, and their fears and forebodings are but too often sadly confirmed. Its occurrence must always prove of deep import to the physician, and his prognosis in the case, especially if unaccompanied by any physical signs, demands careful consideration, and the imparting his opinion to patients requires both caution and circumspection. Perhaps in no other profession are words lightly spoken more liable to misinterpretation. Passing over the obloquy, disparaging comments, &c., which a man throws himself open to by an unguarded expression, the import drawn from it, if unfavourable, can have anything but a beneficial effect on the patient. How often have I heard hospital patients, particularly hysterical young females, who had perhaps on one or more occasions expectorated blood, some coming long distances from the country, seriously and in good faith assert that they were in decline—one lung completely gone, the other affected, and such to have been the opinion of their medical attendants, many of whom physical examination and subsequent treatment proved to labour under no organic disease of the lungs whatsoever. Erring in the other extreme may be attended with still more pernicious consequences. A patient often rendered fool-hardy by the too favourably expressed opinion, emanating perhaps from the certainly judicious desire to keep up his spirits, all misgivings are banished, and with cheerful indifference he again devotes himself to pursuits which originally were the exciting causes of his malady;

hæmoptysis again takes place, and decided consumption of the lungs supervenes. By the term phthisis, or pulmonary consumption—in other words, wasting, or destruction of the lung tissue, I do not wish to be understood necessarily to imply pulmonary tuberculosis. By many, in an indiscriminate manner, these terms are used synonymously, and indeed some, still adhering to the doctrines of Laennec, recognize but one form of consumption, *i.e.*, the tubercular form. Now, in many *post-mortems* of those whose deaths had been the result of wasting and destruction of lung tissue, we find no tubercle, and still it is an undoubted case of phthisis. One case to illustrate the above.

R. S., female, aged thirteen, admitted into the Brompton Hospital March the 29th; mother said to have died of phthisis; had been always delicate; never, however, seriously ailing until four months ago, when her present illness commenced with a cough.

Physical Signs on Admission.—R. Bronchial respiration both anteriorly and posteriorly in upper third; harsh and puerile respiration below. L. *Bruit de pot fêlé* between first and second intercostal spaces; pectoriloquy with gurgling extending down as far as fourth interspace; bronchial respiration below.

Physical Signs, April 25.—R. Gurgling at and above clavicle; amphoric respiration below; humid clicking about third interspace; harsh respiration below. L. Those on admission much increased, with the addition of moist sounds at the base. Died May 3rd.

Autopsy.—R. Several cavities in superior lobe, surrounded by pneumonic consolidation, containing greenish fluid pus; middle lobe in a state of red soft hepatization breaking down, several small cavernuli having already formed; inferior lobe quite recent; pneumonic consolidation; the colour of the superior lobe being pale, middle brick colour, and inferior deep red. L. In a state of thorough excavation, portions at the base being in a somewhat similar condition to the middle lobe of right lung. Both right and left lungs considerably retracted; numerous and very friable pleuritic adhesions; no tubercular deposit discoverable.

The above rather imperfectly quoted case of wasting of the lung, to which one could with difficulty deny the term phthisis, can hardly be qualified as being of tubercular variety. Pneumonic phthisis would be its proper designation. I, for my part, cannot understand why tubercular consumption should monopolize the term pulmonary consumption, when we have other diseases besides

tuberculosis which, when they occur, may cause waste of lung tissue to an equal or greater extent than tuberculosis. Tubercular phthisis, though undoubtedly forming a large proportion of cases of consumption of the lungs, since it does not embrace all, ought to be universally used only as a name signifying a subdivision of pulmonary consumption, and not as one necessarily connected with every phthisical case.

Returning from this discussion, we will resume our subject. The fact that hæmoptysis is not constantly prior or subsequent to any organic disease of the thoracic viscera has been frequently substantiated. Many people during puberty having suffered from repeated attacks of hæmorrhage from the respiratory organs, still arrive safely at the goal of ripe old age, leaving at their death no appreciable pathological signs of their ever having suffered from it. Though such is sometimes the case, we should still regard every case of hæmoptysis coming under our notice with suspicion, and endeavour to arrive at a knowledge of its direct cause. The more obscure this is, the less we should relax in our attempts to discover its origin. Hæmoptysis through mere exsanguinification, except, of course, such a hæmorrhage as would result from either a wound or the bursting of an aneurismal sac, is seldom directly dangerous to life. Though it may occur, as above stated, in persons whose lungs during life afforded no physical signs indicative of its origin, or after death exhibited no traces of its ever having taken place, and though, as several well authenticated cases prove, it may but supplementalize suppressed menstrual, hæmorrhoidal, or other hæmorrhagic discharges, still it must never be looked on with unconcern.

Hæmorrhage from, if we may use the term, healthy lungs, even though it may sometimes relieve nature of a surplus of blood, must, however, have a deleterious effect on the system. In large quantities, and of frequent recurrence, it tends to produce anæmia, one of the most favourable fields for the development of diseases of the chest. Mechanically it at times causes more or less local injury. The extravasated blood in hæmorrhage from the respiratory organs is not always immediately expectorated; the retained portions may form into clots, which act as foreign bodies on their surrounding walls, having quite as great an irritating effect on the parts in their vicinity as a coagulum in a vein would have on the vascular tissue. This I have seen verified by some *post-mortems* of those who had suffered from hæmoptysis a short time previous to

death, in which coagula bearing an apparently complete resemblance to old thrombi of the veins were discoverable in the bronchi. In addition to these circumstances, supposing after one single moderate attack of hæmoptysis, the lungs are completely freed from the presence of extravasated blood, the very process necessary to healing of the blood-vessels from which hæmorrhage has taken place, if similar, as we have good reason to suppose it is, to that which occurs in the large blood-vessels, namely, adhesive inflammation, though for manifest reasons necessary to the vessels themselves, cannot but be sympathized with by the surrounding parts, and often to no good purpose.

Hæmoptysis, as the result of disease, is said to be most constant in its attendance on heart disease, especially that of contraction of the mitral orifice, accompanied by valvular insufficiency. In this form of heart disease more than any other we have the lungs often subject to a considerable pressure of blood, which; in addition to the, in this disease, usual quality of the latter, the relaxed condition of the system in general render the walls of the blood-vessels unequal to the task imposed on them. Hæmoptysis dependent on disease of the heart, though perhaps for the first time of no great importance, and is, as Trousseau expresses it, "less fulminant than bronchial hæmorrhage occurring in phthisis of tuberculous origin," still has a tendency to increase both in volume and frequency as the heart disease approaches its fatal termination. Repeated attacks of hæmoptysis resulting from disease of the heart, in addition to, by depravation of the blood, favouring the production of anasarca and other complications, is, particularly in chronic cases, which cases of mitral valvular disease mostly are, sometimes followed by pulmonary consumption. One case of this kind I remember. The patient, a woman aged twenty-three, with no traceable phthisical hereditary predisposition, had, when thirteen years of age, rheumatic fever, followed by disease of the mitral orifice, for which she had been in hospital, when about twenty-one years old, affording at that time no physical signs of any organic disease of the lungs; had experienced several moderate attacks of hæmoptysis, and during the time I was acquainted with the case softening of the middle lobe of right lung took place with the formation of cavities.

In chronic cases of phthisis pulmonary hæmorrhage is more frequently followed by immediately fatal consequences. Here the circumstance of the often extremely limited area of healthy lung

tissue, capable of oxidizing the blood, being interfered with to any extent, must terminate life. This happens most commonly in either of two ways, viz., either simply direct suffocation from the mass of extravasated blood occupying the air passages and cells of what remains of healthy lung tissue, which does not possess sufficient power to reject it, or else through secondary inflammation in that part, occasioned by retained portions of blood acting as foreign bodies.

Besides the many exciting causes for hæmoptysis in phthisis commonly given by authors, I am convinced that it is often the effect of (if such an expression is allowable) the too rapid improvement of patients, the making more blood than the limited space of the breathing surface is sufficient to aerate, thus effecting an hyperæmic condition of the lungs, which relieves itself by hæmoptysis. This is of common occurrence in hospital patients, who, on their entrance into such an institution, especially into one devoted to diseases of the chest, often change for a generous nourishment, for an equable temperature, for a life for the time being devoid of many of the petty cares of this life, an abode little better than a cabin, with all its concomitant miseries. As bearing this out I call to mind two cases, one a fine young unmarried woman, with upper portion of both lungs in the third stage of phthisis, who had been an inmate of the Brompton Hospital for three months, whose chest symptoms had during that period been sensibly ameliorated, and whose weight had in that time been increased by 24 lbs., and who had herself felt so much improved that she doubted of many of her friends being able to recognize her on her return home. With but little warning hæmoptysis set in, followed in the course of thirty-six hours by an acute attack of capillary bronchitis, which put an end alike to her doubts and existence. The other, a married woman, also with the superior portions of both lungs excavated, intended to have left hospital the next day, with chest symptoms greatly improved, no moist sounds, and weight considerably increased, was suffocated by a sudden attack of hæmoptysis. In these cases, no doubt, the weakened or diseased blood-vessels, unable to support the additional strain imposed upon them by the increased quantity of blood, gave way, and were followed by fatal consequences. However, cases of hæmoptysis during phthisis occur, both frequent in repetition and large in volume, without having any very apparently immediate deleterious effects on the patients, except that of weakening them for the time. One case

with an enormous cavity in left lung, who for three months out of three consecutive years had been an inmate in Brompton; expectorated during those nine months about 5 pints of blood. The first year her weight was 128 lbs.; the second, 125 lbs.; the third year, 124½ lbs., and physical examination showed during those several years but slight increase in the disease of her lungs.

The portion of the respiratory organs from which hæmorrhage in hæmoptysis takes place has been frequently the subject of dispute. Dr. W. Hayle Walsh denies that bronchial hæmorrhage is common, and he asserts that in the majority of cases, when blood in any quantity has made its way into the bronchial tubes, even when heart disease is present, it will be found that the parenchyma of the lung, and not the mucous membrane, has given way molecularly. Niemeyer, on the other hand, states that bronchial hæmorrhage is by far the most common, while Trousseau, taking a middle course, considers that hæmoptysis in phthisis occurs ordinarily from the surface of the bronchi, but that hæmoptysis in heart disease is more generally parenchymatous, viz., taking place first in the air-vesicles of the lung.

That it is generally parenchymatous, accompanying heart disease, is but natural to suppose; but occurring independently of that disease from the lung substance, must but very seldom be complicated with organic disease of the pulmonary tissue.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

State Medicine: A Discourse, delivered before the University of Dublin, on Saturday, April 6th, 1872. By WILLIAM STOKES, M.D., D.C.L., F.R.S., Regius Professor of Physic. Dublin: John Falconer. 1872.

WE recommend even those who had the advantage of hearing Dr. Stokes's discourse delivered to read it. The subjects with which the speaker dealt were so numerous that he was unable to do more than mention some of them, and consequently the discourse in its delivery probably made less impression on the minds of those who heard it than if it had been devoted to one or two of the many questions with which State Medicine deals, and we are very glad that we now have it in a permanent form, as it gives at once a sketch of the progress of Sanitary Science in Great Britain and Ireland, and points out in an eminently suggestive way the various directions in which Sanitary Science may operate, the objects it sets before it for accomplishment, the good we may reasonably expect from it, and some of the fallacies into which Sanitary Reformers have fallen. Dr. Stokes shows very clearly how erroneous is the idea that the mere accumulation of filth must necessarily produce epidemic disease, and that the disposition to attribute fever and cholera to such a cause is an instance of the tendency of the mind erroneously to attribute great phenomena to too limited a source. He points out that practically the duty of a sanitary officer is rather to endeavour to keep up the health of the population, and so fit them for resisting epidemical influences than to attempt to stop epidemic disease when it comes by extinction of its germs or otherwise. "In the present state of our knowledge," says Dr. Stokes, "are we to hold that preventable influences are the originators of disease? No doubt, civilization demands that all things injurious to health, or noxious to the senses, should not be permitted to exist. But the question remains whether, leaving the origin of disease undetermined, sanitary reform does not act as much by the improvement of the health of the population as by the lessening or actual extinction

of the exciting causes of zymotic affections. The community being better prepared to resist the advent of disease, its spreading will be influenced, and its severity lessened, when it does come.

“This, I apprehend, is the safe and practical way of looking at sanitary reform. It is fortunate that theoretical questions in no way touch the working out of such reform. Questions as to whether the spread of cholera is influenced by the dryness or the moisture of the air; as to the spontaneous generation of germs; or as to whether, when the sewerage of a town is spread out upon the fields, there may be a struggle for existence among various organisms, so that the cholera molecules die out. Such questions only divert the attention from more important matters. The sanitary reformer is not to wait for the advent of epidemical disease. It is rather when a country is free from such that he can best work in removing or mitigating all those causes which experience shows to act against the health of man.”

On the question who should be the future health officers—a question of much moment to many of our brethren through the country—we are glad to find Dr. Stokes has formed a very decided opinion.

“The proposal to employ the Poor-law medical officers as officers of health has been objected to by some able writers on two grounds principally: one, that the education of these gentlemen has not been sufficient to enable them to deal with questions of State Medicine, and the other, that from their too often dependent position in relation to members of local boards, they will be crippled as to their action for the public good. This last objection would, I believe, apply far less to Ireland than to England.

“But granting that they have some foundation, what is the proper way of meeting them?

“It may be admitted that in many rural districts the education of the dispensary medical officer, sufficient to qualify him for being placed on the register, is often so completely technical that he cannot do more than deal with curative medicine as he best may. Should such a man be called to report on questions of State Medicine, he will probably be found at first at least, defective. But necessity is a great instructor, and it is in the nature of things that he will every year improve. Self-education can do wonders. He will be assisted by skilled inspectors, and his good sense, probity, and honourable ambition will do the rest.

“There will be a difficulty where the Poor-law medical officer is in a dependent position. But this is one of those difficulties which

must gradually disappear with the advance, on the one hand, of public education in social and sanitary science, and, on the other, in the training of the future practitioner in that extra professional knowledge which will raise his social position, and be sought for otherwise than by a technical education."

At a time when little attention was given to the subject of sanitary science Dr. Stokes took it up. His exalted position as Regius Professor of Physic in the University of Dublin, enabled him to give an impetus to its study, and mainly we have no doubt through his influence the Board of the University were induced to establish a diploma in State Medicine. With it his name will ever be inseparably connected, just as in former years it was connected with the physical diagnosis of diseases of the chest, and with the study of the essential fevers.

Syphilis: its Nature and Treatment, with a Chapter on Gonorrhœa.

By CHARLES ROBERT DRYSDALE, M.D., Member of the Royal College of Physicians of London; Fellow of the Royal College of Surgeons of England, &c., &c. London: Baillière, Tindall, and Cox, 20, King William-st., Strand. 1872. Pp. 165.

DR. DRYSDALE'S book is well worthy of careful perusal. It is a work which we can conscientiously recommend to our readers. It is thoroughly honest; great labour has evidently been expended upon it; and it seems to us to be executed with a degree of impartiality in the highest degree creditable to the author. Dr. Drysdale's views are strongly anti-mercurialistic, yet he assuredly does not leave it in the power of any adversary to say that the authorities on the other side are not fully and freely quoted. It is a work characterized by an amount of research only equalled by the frankness and candour with which the views of others are set forth. Yet, having read it carefully, we must confess that a sadder undertaking of the kind we have not for a long time been called upon to perform. The chaos of conflicting opinions is bewildering. We naturally ask, have men, after centuries of study and observation, done as yet so little towards solving this mystery? Dr. Drysdale has brought together a valuable body of evidence, but he has done little to separate the chaff from the wheat. This is the difficulty. We do not say that he has not made as good, if not a better, attempt than others. But who can analyse the heap of undigested evidence, and wild, reckless

assertions which have for ages been accumulating on this subject. We want facts, observations made with scientific precision, cases, the details and results of which are known for years. In this respect Dr. Drysdale gives little information. The merit of his work lies in another direction—honesty and literary research are his strong points.

Gonorrhœa is treated of in the early chapters, first, as it is met with in the male. The complications of gonorrhœa and stricture of the male urethra are entered into at some length. A chapter is devoted to gonorrhœa in the female. Mr. Morgan's views on the propagation of venereal disease from gonorrhœa are stated, although but little evidence, either *pro* or *contra*, is adduced:—

“In a pamphlet called ‘A New View of the Origin and Propagation of Venereal Diseases, 1870,’ Mr. Morgan says, ‘it appears that the product of the vaginal discharge of a patient suffering from syphilitic infection, is a chancroid or soft sore, when the discharge is introduced under the skin or applied to an abraded surface.’ He adds that, ‘a more remarkable power possessed by this vaginal secretion is the production of a chancroid by inoculation on the patient's own person;’ and that he has on several occasions taken the secretion as wiped from the os uteri and inoculated with it unsuccessfully, while from the vaginal discharge he has succeeded. He has also inoculated from the vaginal secretion of cases of uterine ulcer, and from the ulcers themselves without any result. He adds, and the author can bear his own testimony to this, that he has also ‘inoculated without result from the gonorrhœal discharge of the male.’ Mr. Morgan remarks that it seems necessary that the patient ‘be suffering from the earlier stages of constitutional infection, whether as yet latent or undeveloped, so as to be transmitting a sore by inoculation from the vaginal secretion.’ This is a very important point in the doctrine of the chancre, for M. Ricord, in his ‘Lectures on the Chancre,’ page 44, says that ‘a person with syphilis, who contracts a soft sore, may by means of this transmit syphilis.’ There exist already, according to Ricord, as quoted by Mr. Morgan in page 8 of his pamphlet, ‘a certain number of observations which tend to prove that the soft chancre of a syphilitic subject may also transmit itself in its own species, that is to say, as a soft chancre;’ and Mr. Morgan remarks that, in all the inoculations he has recently made, the product from the artificially generated chancroid was a chancroid, though generated from and on syphilitic subjects. ‘The same discharge from the vagina,’ he says, ‘would succeed in some cases and not in others; and, though failing to produce a specific pustule one day, it would succeed on another.’ The following case is cited by him, and may serve as an example of what he has observed and narrated; it

is contained in page 8:—‘On the 23rd of May, the first auto-inoculation was performed from the patient’s vaginal secretion. The patient was intensely affected with syphilis, and her vaginal discharge was inoculated sixty-three days after her admission, causing the appearance of a chancroid. On June 20th, twenty-eight days after the first inoculation, and from two to three weeks after the healing of the primary sore, he successfully inoculated another patient infected with syphilis, from the vaginal discharge of the same patient.’ Mr. Morgan says with truth that ‘this is to be borne in mind, with respect to the conduct of inspections under the Contagious Diseases Acts, as it might be supposed that freedom from primary or actual sore obtained freedom from contagion, yet a vaginal discharge in a syphilitic patient might be overlooked.’ Of course, these experiments of Mr. Morgan’s are too recent to have been tested by other careful observers; and it is scarcely possible to say what may be the effect if the facts he narrates are proved to be consonant with nature. In two instances in syphilitic women, the author has tried the effect of inoculation of gonorrhœal discharge quite recently, without any chancroid being produced; but in another, made before that time, a pustule formed. It is premature to make any further remarks on the interesting cases cited by Mr. Morgan; they will, doubtless, soon be carefully repeated by others, now that continental wars seem over for a time, which all lovers of human happiness may trust will be one of lengthened duration, although the future looks gloomy indeed.”

The greater portion of Dr. Drysdale’s work is devoted to the consideration of syphilis. The doctrine of the chancre, the causation and diagnosis of syphilis are learnedly and fully discussed. Two elaborate chapters are devoted to the treatment of syphilis, and the most opposed doctrines are fairly stated, and authorities are quoted in a fashion positively bewildering. An appendix is added, containing the latest details on the doctrines of syphilis, and in which the question of the unicity or duality of venereal sores is canvassed.

Remarks on the Prevalence and Distribution of Fever in Dublin.

By THOMAS W. GRIMSHAW, M.D., &c. Dublin: Fannin and Co., Grafton-street. 1872. Pamphlet, pp. 36.

THIS is a very able and suggestive pamphlet, and its subject matter is most opportune at the present moment, when a loathsome epidemic is raging in the city of Dublin.

The author shows that the form of fever which sanitarians usually regard as the most preventible by hygienic means, is increasing instead of diminishing in Dublin.

Dr. Grimshaw, after carefully studying the distribution of fever throughout the city, comes to the conclusion that its prevalence in certain districts is due to local unsanitary conditions, which ought to be under the control of the Civic Public Health authorities. He proposes to re-organize the sanitary department of the Corporation in the following manner:—

“1st. That the city is already divided into districts, for the purpose of medical attendance on the poor, registration, and vaccination.

“2nd. That the Dispensary Medical Officers are the first to know of sanitary defects, or of the outbreak of epidemic.

“3rd. That it is the direct interest of the Dispensary Medical Officers to maintain their districts in the best sanitary condition possible, as their work is thereby much diminished, and the difficulties and dangers of attending on the sick reduced to a minimum.

“There are seven Dispensary Districts in Dublin, each of these have two Medical Officers, and to each a Sanitary Inspector should be attached.

“This would give the following sanitary organization for the city of Dublin, and I have made a rough estimate of the cost of such an organization.

“A Health Officer, who should, if possible, be attached to a large Hospital, as a Physician or Surgeon, but who should not engage in private practice, at a salary of £600 a-year, - - - - -	£600
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“Fourteen District Medical Officers, who should, of necessity, be the Dispensary Medical Officers of their respective Districts, at salaries of £50 a-year each, - - -	700
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“Seven Sanitary Inspectors, one for each of the seven City Dispensary Districts, at salaries of £40 a-year each, -	280
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“A City Analyst, at a salary of £150 a-year, with all fees for Analysis, - - - - -	150
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“A Secretary to the department, at a salary of £200 a-year, -	200
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Total, £1,930

“I believe this organization might, with safety, be extended so as to include the Suburban Districts, and thus bring the whole Dublin Districts within the control of the Chief Health Officer.”

Dr. Grimshaw's scheme certainly appears to be a feasible one, and one, too, which would work satisfactorily.

Sciatica, Lumbago, and Brachialgia; their Nature and Treatment, and their immediate Relief and rapid Cure by Hypodermic Injection of Morphia. By HENRY LAWSON, M.D.; Assistant Physician to St. Mary's Hospital; and Lecturer on Physiology in St. Mary's Hospital Medical School. London: Hardwicke. Pp. 200.

HAVING read, with considerable interest, the articles on sciatica by Dr. Lawson, as they appeared some two years since, in the columns of the *Medical Times and Gazette*, we have much pleasure now in directing attention to their publication in a collected form, with the addition of chapters on lumbago and brachialgia, as well as a review of the opinions of some eminent authorities on these allied affections. We trust that those who may be still unaware of the great advantages to be derived from the mode of treatment advocated by the author, or who may hesitate to carry it out in its entirety, will, after a perusal of the arguments and facts so forcibly brought forward by Dr. Lawson—himself at one time a fearful sufferer from sciatica—be convinced of its value and efficacy.

Dr. Lawson is aware that in recommending his plan, he exposes himself to the charge of treating a symptom only; but, in the case of sciatica, he believes that "the symptom (pain) is the whole disease; and by arresting it we remove the affection." He is of opinion that at present we know nothing of the pathology of sciatica, and that its connexion with that vague class of diseases, neuralgia, is questionable. He is also strongly opposed to the theory that sciatica is an affection of the central nervous system, as its classification among the neuralgiæ by some authors would seem to indicate, but suspects that the portions of the nerve structure primarily attacked, are the delicate filamentous reticulations on the surface of the sarcolemma.

Upon the subject of the treatment of sciatica, the remarks of Dr. Lawson are entitled to weight, as he speaks from personal experience in his own case, of the value of nearly every remedy that has ever been lauded as a cure for the disease; and the conclusion he comes to is, that "the true and *almost the only remedy for sciatica*, is hypodermic injection of minute quantities of morphia." The solution our author recommends, differs somewhat from those in ordinary use; it is simply composed of ten grains of hydrochlorate of morphia, dissolved in two drachms of distilled water, without

any acid or glycerine. This preparation is solid at ordinary winter temperatures, and generally so in summer, and, consequently, must be heated before each injection; it possesses, however, the advantage that it escapes decomposition, which so frequently occurs in other solutions. One-sixth of a grain of morphia, *i. e.*, two minims of this solution, is the proper commencing dose. The effects produced by the operation—which it is well to perform immediately after a meal, and in the locality of the pain—are: “absence of pain, intense comfort for a time, prolonged sleep, and increased appetite.” These results seldom last longer than twenty-four hours, and it will be well then to repeat the dose, and, if requisite, increase its strength. In severe cases, it will be necessary to inject twice a day, and the hypodermic use of morphia may have to be continued for many months. The prevention of the pain of the injection—with some timid patients an important point—may, as the author reminds us, be secured by producing a moderate degree of anæsthesia of the skin by means of the ether-spray apparatus.

Dr. Lawson does not seem to have observed any bad results from the employment of morphia in this form, beyond a sensation of nausea experienced by some after the operation, which, he states, may be relieved by taking food. We regret, however, that he has given no caution against the danger which the prolonged use—or rather abuse—of the hypodermic injection of morphia, may sometimes produce. Although temporary immunity from suffering is always procurable by a sufficiently large dose, yet, in some protracted cases, there is, when the influence of the morphia has passed off, a return of the pain, and a craving for a repetition of the injection, which engenders a “morphia-habit,” difficult to abandon, and which, from a reliance upon the hypodermic treatment only, may possibly, as suggested by Dr. Clifford Allbutt,* “perpetuate the morbid condition under the mask of a seeming relief.” It must not be supposed, however, that although Dr. Lawson believes that 90 per cent. of the cases of sciatica can be cured by the hypodermic injection of morphia alone, he ignores altogether the assistance to be derived from other sources. Cod-liver oil, iron, and quinine, he has found very valuable, in “that they help to maintain the nutrition of the body, which the agonizing pain of the malady so greatly interrupts.” The employment of alcohol—whether as brandy or whiskey—in severe sciatica, is, the author says, “to be uniformly insisted on;” but beer, wines, tea, and coffee, are to be avoided.

* Practitioner, Vol. v., p. 330.

In the treatment of lumbago and brachialgia, Dr. Lawson has found the hypodermic injection of morphia as useful as in sciatica.

Herpes zoster, which so frequently accompanies lumbago, and intensifies the sufferings of the patient, is also relieved by the hypodermic injection of morphia, and by painting over the vesicles, immediately they appear, with styptic colloid.

Dr. Lawson's book, although published with the evident and laudable intention of making more generally known a means of employment of a single remedy, conveys, at the same time, such an amount of information on the subjects treated of, and is written with such evident candour, especially as regards other modes of treatment, that we have no hesitation in recommending it to our readers.

Photographic Clinique of the British Hospital for Diseases of the Skin. Edited by BALMANNO SQUIRE, M.B., F.L.S.; Surgeon to the Hospital. London: 56, Great Marlborough-street, Regent-street, W.

MR. SQUIRE is already known as a contributor to the artistic side of dermatology by his coloured photographs of skin diseases, and he now, under the above title, commences a new series, in quarto, to be issued every three months. No. I., which lies before us, represents very clearly the features of an interesting case of spontaneous oval keloid, three inches long, occurring on the chest of a man, aged 31, and strongly reminds us of a keloid tumour which came under our observation three years ago, and which was situated upon the summit of the right shoulder of a lady. We trust that Mr. Squire will be encouraged to extend his illustrations which, being photographic, are necessarily accurate in outline, and, when carefully coloured, are of peculiar value as aids in the diagnosis of *rare* forms of disease. In future numbers it would be well to insert the name of the disease at the foot of the illustration, as is done in the admirable plates of the New Sydenham Society, and to alter the present inelegant title.

Sull' Alcoolismo Acuto Considerazioni. Del Cav. Dottor EZIO CASTOLDI.

Reflections on Acute Alcoholism. By Dr. EZIO CASTOLDI. Milan, 1871. Pamphlet, pp. 84.

THE object of this memoir is to give a classification of the different mental disorders caused by the abuse of alcohol, as the division of the subject by Magnus Huss and others, into acute and chronic alcoholism, does not, in the opinion of the author (who appears unacquainted with Dr. Anstie's division of acute alcoholism into four forms) embrace all the varied psychoses caused by alcoholic intoxication. The first mental alteration (after drunkenness), which Dr. Castoldi recognizes as being caused by the abuse of alcohol, he terms "acute delirium of tipplers." This he distinguishes from delirium tremens, mainly by the absence of hallucinations and tremors, and considers to be the *immediate* effect of alcohol on the brain, delirium tremens being a *mediate* result of the poison.

Acute delirium, he says, is more frequent after indulgence in red wines than in spirits, and occurs in strong healthy persons, rather than in weak ones; it is always preceded by drunkenness, while delirium tremens, on the other hand, frequently manifests itself in individuals who drink hard without ever getting intoxicated. The author acknowledges that cases of acute delirium, uncomplicated with delirium tremens, or with some symptoms of chronic alcoholism, are rare; but affirms that the affection he describes is a "true nosological entity," not a mild form of delirium tremens; and cites the action of certain articles in therapeutics (opium, tobacco, &c.) as analogous to the mediate and immediate effects of alcohol according to his theory.

Cases illustrative ⁽¹⁾ of acute simple delirium; ⁽²⁾ of acute delirium complicated with delirium tremens; and ⁽³⁾ cases in which habitual topers (*bevitori di professione*) in a state of chronic alcoholism were attacked, follow; and in the remainder of the pamphlet, the second form of acute alcoholism, delirium tremens, is discussed. Among the predisposing causes of the latter disease are enumerated, as if with an air of originality, the influences which Dr. Anstie, some years since, called attention to—viz., the tendency which the children of families, in which insanity is hereditary, or of drunkards or epileptics, or of parents who have suffered from

other severe forms of nervous disease, have to become victims to intemperance. As an exciting cause, the author states that repeated attacks of delirium tremens may be induced by constant exposure to *vinous emanations* alone, without any indulgence in the wine itself(?) The influence of such a cause may, perhaps, be a satisfactory excuse for some unsuspecting employers of drunken cellar-men and pot-boys; but to our minds, we confess, would suggest a greater confidence on the part of the observer, in the veracity of the patient, than we would be inclined to put in his integrity. The statement that British soldiers in India, suffering from delirium tremens, are treated by being wet-cupped at the nape of their necks, and have calomel given to them until they are salivated, is one to which it is hardly necessary, in behalf of the present generation, at least, of military medical officers, to give an unqualified denial.

Although Dr. Castoldi has shown but an indifferent acquaintance with the present state of knowledge, as regards acute alcoholism, his paper is an interesting one, and is illustrated by numerous concisely related cases. We hope, however, that in his promised forthcoming memoir on chronic alcoholism, he will compare his observations, which, coming from such a quarter as the Ospedale Maggiore of Milan, are worthy of attention, with the works of some more recent authorities than those referred to in the present publication.

Worms; A Series of Lectures on Practical Helminthology; delivered at the Medical College of the Middlesex Hospital; with Cases Illustrating the Symptoms, Diagnosis, and Treatment of Internal Parasitic Diseases. By T. SPENCER COBBOLD, F.R.S., Honorary Correspondent of the Academy of Sciences of Philadelphia. London: J. & A. Churchill, 1872. Pp. 178.

DR. SPENCER COBBOLD is already so well known as an authority on entozoa, that we were very glad to see a work from his pen in which the subject of human parasites was discussed in its clinical aspect, and we have attentively read the lectures now before us. From the number of cases alluded to in them it is evident that the author, although chiefly known by his investigations into the natural history of the entozoa, has also had considerable experience in the treatment of the diseases they produce. The greater part

of the work is devoted to tapeworms. In dealing with them Dr. Cobbold has found the old remedy, the male-fern, especially when the freshly and carefully prepared extract was used, the best remedy.

“The above cases,” he says, “offer every encouragement to those who administer male-fern remedies in a proper manner; for although in the treatment of the pork and mutton tapeworms you may occasionally be baffled, it is satisfactory to know that, with proper care, you are not likely to be otherwise than successful with the unarmed variety derived from beef. This will be made more obvious immediately. Meantime, let me call your attention to all the more important drugs commonly employed. This I shall do in terms very similar to those I have adopted in the small treatise previously referred to. ‘The remedies for tapeworm are male-fern, kousso, kamala, turpentine, panna, pumpkin-seeds, and pomegranate-root bark. The right administration of any one of these is likely to produce the desired result; but many other drugs have been and still are employed with more or less success. Patients have consulted me after trying five or six of the above-named remedies, to say nothing of others not worth mentioning. Some Practitioners, as I have said, seem to think one vermifuge as good as another. Because they find *santonin* a useful remedy in threadworm, and almost a specific in roundworm, it by no means follows that the same drug is effective, or even of the slightest value, in tapeworm. When so many excellent tapeworm vermifuges abound, it becomes a waste of time to dwell on the virtues of second and third-rate drugs, such as oxide of silver, tin, scammony, jalap, and various other drastic purgatives which exert no poisonous influence upon the worm.’ Dr. Shapter, of Exter, has employed chloroform with success; at least, speaking of one or more cases thus treated, he says, ‘The cure was effectual, and without distress of any kind.’ Now, supposing it was really necessary to try several drugs in succession, I would recommend their adoption separately in the order I have just placed them. Of course in no individual case will you ever be called upon to indulge in such a practice. Without asserting their actual relative value as tapeworm poisons, I give them a preference in the order in which they are here recorded. Probably, as I have elsewhere said, there is no better remedy for tapeworm than oil of turpentine, and yet its nauseous character, combined with the fact that it not unfrequently produces irregular and violent effects upon the nervous system, are circum-

stances always inducing me to substitute other drugs. From cases which have come under my notice I have no doubt whatever as to the great anthelmintic virtues of turpentine."

The author next gives a series of cases illustrating the symptoms and treatment of the seatworm, the *oxyuris vermicularis*, and of the roundworm, the *ascaris lumbricoides*. In expelling the former he has found enemata of tansy and quassia, with the addition of two or three drachms of sulphuric ether, very effectual; but in obstinate cases he uses salines by the mouth, and recommends a course of the mineral waters of Friedrichshall and Pullna. For ejecting the roundworm he has found, as most of us have, that santonin is the most reliable drug. Having disposed of these, the most common human parasites, Dr. Cobbold narrates some cases in which insect-larvæ escaped from the human intestine, and gives an interesting account of a case which he had the opportunity of closely watching, in which hæmaturia (the "endemic hæmaturia" of Natal and of Egypt) was caused by the fluke called by him the *Bilharzia hæmatobia*, in honour of Dr. Bilharz, of Cairo, who discovered it.

Before concluding our notice of Dr. Cobbold's lectures, we feel bound to state our conviction that, with the thorough knowledge of the subject which he possesses, he could very easily have given more information in less space. The book is written in a style with which the works of other specialists have made us familiar, and which we suppose is found necessary. We hope many of those who have worms, or who imagine they have, will avail themselves of his special skill in the recognition and the ejection of such troublesome tenants; but as reviewers we cannot but regret that while the author devotes so large a portion of the space at his disposal to cases many of which have nothing in them worth recording, he should refer us for useful information to his other works, and dismiss such an interesting and important subject as trichina is, with the assertion that, tempting as it is, he cannot dwell longer on it.

PART III.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

Wednesday, April 24th, 1872.

HENRY EAMES, M.D., Honorary Secretary.

DR. AQUILLA SMITH, in the Chair.

On the Application of Gases as a Means of Destroying Contagion. By CHARLES A. CAMERON, Ph.D., M.D., L.K. & Q.C.P.; Prof. of Hygiene in the Royal College; Lecturer on Chemistry in Steevens' Hospital Medical College, and the Ledwich School of Medicine; Analyst to the City of Dublin.

It is impossible to discuss in a scientific manner the subject of disinfection without at the same time considering that of the intimate nature of contagion. Enormous quantities of bleaching powder, carbolic acid, and other substances are annually used in these countries as sanitary agents. They are often solely employed for the purposes of preventing putrefaction; but no inconsiderable proportion of the quantities of "disinfectants" used is applied for the purpose of destroying the *materies morbi* of enteric and other fevers, small-pox, cholera, and other zymotic diseases.

During the decomposition of animal and vegetable substances, sulphuretted hydrogen, phosphoretted hydrogen, and other offensively odorous gases, as well as vapours, and probably solid particles, are evolved. The continuous inhalation of these æriform products of the retrograde metamorphosis of organic bodies may, like intemperance and deficient nutrition, lower the vital powers, and thereby render the animal mechanism less able to resist the influence of small-pox, cholera, and similar maladies when they are epidemic. There is, however, no evidence of a satisfactory nature to prove that the ordinary emanations from decomposing

animal or vegetable substances are the cause, *per se*, of any contagious disease.

If, during the decomposition of ordinary animal and vegetable substances, matters were evolved capable of producing fevers and other zymotics, it is difficult to conceive how the population of towns could escape complete extirpation from those diseases.

It is more reasonable to believe that zymotic diseases are each of them produced by the introduction of a specific *virus* or *germ* into the animal economy. It is probable that some zymotics are caused by germs which are incapable of multiplication in the body. Such diseases are not, therefore, contagious in the ordinary sense of that word—that is, they are not propagated by matters which are cast forth from the bodies of the sick. In the valuable Report on Yellow Fever, prepared by Dr. J. C. Nott, and published in the Annual Report of the Board of Health of the City of New York, for 1870-71, very strong evidence is adduced to prove that yellow fever is caused by germs, which are not bred within the body. It is, however, shown that those germs may be transported from place to place in the clothes and baggage of men. Dr. Nott brings forward the most convincing proof that decomposing organic matter does not, *per se*, produce yellow fever; and shows that the germs which cause the malady are devitalized by exposure to a temperature of 32° Fahrenheit.

It is very easy to destroy the unpleasant matters that are given off during the decay of organic bodies. These matters obey the physical law of the diffusion of the gases, and spread rapidly and equally throughout the atmosphere. When they are generated in, or enter, a room, they are readily rendered innocuous by admixture with a small proportion of chlorine or sulphurous acid gas. The use of disinfectants is, therefore, to be commended, because they preserve the atmosphere free from malodorous gases and vapours. Some kinds of so-called disinfectants^a are also of great utility as a means of preventing the putrefactive decomposition of organic substances. These disinfectants are properly termed antiseptics. They do not altogether prevent animal and vegetable matters from decay; but they greatly retard that process, and then decomposition without sensible putrescence only takes place. It is well then to put into our sewers and other places containing effete organic matter such substances as bleaching powder, which destroy the fœtid products of too rapid decay, or such compounds as carbolic or chromic acid, which prevent these hurtful products from being formed, except in almost infinitesimal quantities.

The application of disinfectants for the purpose of destroying the

^a This term is used in its popular signification, and includes those substances which, like nitrous acid, destroy noxious matters, or which, like carbolic acid, act chiefly as antiseptics.

actual *contagium* of certain diseases is the most important use to which these substances are applied. And here the question arises—what is the physical nature of contagion? If we have not some clear conception relative to this point, it is evident that our use of disinfectants is merely empirical.

What is it that we try to destroy when we generate chlorine gas in a room which has been tenanted by a small-pox patient? Is it a gas, or a vapour, or an abnormal condition of one or more of the ordinary constituents of the atmosphere? If the cause of the disease lies in an abnormal condition of the atmosphere—in the occurrence of a “pandemic wave” in that fluid, the disinfection of the air of a particular room, would be useless, because, where ordinary ventilation is adopted, the purely gaseous contents of an apartment are wholly renewed many times in an hour. What, therefore, would be the use disinfecting a room if the atmosphere, on entering it, be already tainted! There are many physicians who believe that epidemic diseases are caused by an abnormal condition of the atmosphere; but even those, or at least the majority of them, admit that they may be propagated directly from the sick to the healthy. Who can deny that the matter taken from a small-pox pustule will produce small-pox, if introduced into the blood of a healthy man? It is clear that in this case a palpable agent produces the disease, and the observation of mankind during countless ages has incontrovertibly established the fact that some diseases are communicated from individual to individual. If cholera, small-pox, rinderpest, and other zymotic and epizootic diseases are caused by abnormal atmospheric conditions, why is it that they speed along the highways of commerce, that they spread most rapidly as the density of population increases, and that they prevail most in those places where least attention is paid to the removal of organic filth? If the amount of carbonic acid in the atmosphere were increased from its normal proportion of 4 parts in 10,000 parts of atmospheric air to 4 parts in 100, serious disease would be the result; but it would afflict all classes alike, and would ravage the country regions equally with the urban districts.

A careful examination of acknowledged facts relative to nearly all the more important epidemic diseases fully justify the belief that each is produced by the introduction of a *materies morbi*, or germ, or virus, or some palpable substance from the bodies of the sick into those of the healthy; and by that way alone. This view of the mode of propagation of zymotic diseases is, perhaps, most conclusively proved by admitted facts in relation to two contagious diseases—namely, *scabies*, or common itch, and syphilis. Is either of these diseases ever produced by atmospheric causes? Who would be believed if he stated that he caught syphilis from the air? True, we have not isolated the actual poison of syphilis; but we know that an extremely minute quantity of a liquid, containing solid particles,

includes this poison; and it is further clearly established that sporadic cases of syphilis do not occur in our time. With respect to common itch, it was proved long ago that the disease was produced by a small insect, and that it was propagated from individual to individual. If all the *acari scabiei* and their *ova* now in existence were destroyed there would be an end of the itch for ever. Huxley, who is by no means an ultra vitalist, admits that there is no evidence of spontaneous generation occurring in our time.

The itch is a good example for the purpose of illustrating the nature of contagion. The *materies morbi* is easily seen; it is an entity, it possesses reproductive powers, begetting its own kind, and it is never found except in the bodies of higher animals. The non-contagionists must admit that at least in the case of this disease the theory of the contagionists is proved to demonstration, and simply because the virus of the disease is so large as to be almost seen by the unassisted eye.

The weight of evidence and of opinion too, in the case, at least, of epidemiologists, is in favour of the germ theory of zymotic disease, but most important problems relative to the intimate nature of the different contagia and to their co-relation, are still to be determined. In general the contagious matter appears to be excessively minute. Chauveau (*Comptes Rendus*, October 19th, 1868), diluted the liquid taken from the pustules of sheep-pox with 10,000 parts of water, and found that it still retained its power of producing small-pox in the sheep. Vaccine matter from man may be diluted with ten times its weight of water without losing its contagious property to a sensible extent, but if diluted with 500 parts, it becomes perfectly inactive. Hence it is evident that the contagious liquid of sheep-pox is many times more powerful than vaccine, probably because it contains a larger number of the actual particles, or germs that produce disease. These germs have been carefully sought for by such eminent pathologists and microscopists as Béchamp, Estor, Cohn, Nägeli, Hallier, Chauveau, Sanderson, De Barry, Thomé, Klob, Hoppe-Seyler, and Lionel Beale. On the whole, the results of the investigations of these inquirers have not been barren. It is shown that vaccine contains, in suspension, minute quantities of two kinds of solid particles—*leucocytes* (which resemble pus corpuscles), and smaller particles not exceeding the $\frac{1}{20000}$ of an inch in diameter. The leucocytes may be easily separated from the other particles and the serum; and they are found to be perfectly inactive. The vaccine property must, therefore, reside either in the small particles or the clear serum. By means of the diffusion apparatus, Burdon-Sanderson and Chauveau have succeeded in obtaining the serum free from the small particles, but failed to produce vaccinia with it either in man or in the ox. These important and accurately conducted experiments prove that the actual cause of

cow-pock, and inferentially of other kinds of small-pox is a minute solid and insoluble body.

In liquids containing decomposing organic matter we usually find large numbers of minute living beings in a state of great activity. Some are spheroidal, others resemble knotted rods. The former are termed monads: *micrococci*, or *microspores*; the latter are called *bacteria*, *zooglæa*, *vibriones*, &c. The micrococci are each about $\frac{1}{20000}$ of an inch in diameter; they move about with great rapidity, and multiply by cell division. When they elongate into rods, they acquire a peculiar vibratory movement, which has led them to be termed vibriones. Béchamp and Sanderson include under the generic name, *microzyme*, both the spheroid particles and the rod-like bodies, into which they are developed. No doubt there are numerous varieties of microzymes, but the minuteness of these infusorial animals renders it extremely difficult to discriminate between the different species.

Hallier regards the bacterium, notwithstanding its activity, as a plant. He asserts that the micrococci or their germinal matter exist in all contagious liquids. It is remarkable that Sanderson did not detect either microzymes or their germinal matter in ordinary pus, whereas in pyæmic pus he found swarms of these objects.

It has been strongly urged as an argument against the germ theory of disease that it fails to account for epidemics. Why should small-pox die out in Ireland, and then suddenly reappear and rage with great violence in many parts during the last twelve months? How is it that cholera periodically invades the west from the east? Why does an epidemic gradually increase in intensity, attain a maximum of virulence, and then gradually die out? It is difficult to answer these questions satisfactorily, because all the factors concerned in the propagation of zymotic disease are not known. The anti-contagionists contend that small-pox and similar diseases are propagated by other means than by emanations from the bodies of persons suffering from the diseases, and they believe that at particular times the condition of the atmosphere and of the constitution of the population are peculiarly favourable to the spread of these maladies.

If it be admitted that small-pox and certain other diseases are sometimes caused by matters thrown off from the sick making an entry into the bodies of healthy persons, then the phenomena of epidemics may be shown to be explicable without abandoning the theory that small-pox (and some other diseases) are only communicable from individual to individual. We can readily understand that the low forms of life which produce epidemic and epizootic disease might, under favourable circumstances, multiply to a greater extent than usual. Under such circumstances the chances of their getting into the bodies of animals would be propor-

tionately increased, and a local epidemic would be the result. Intercommunication between the place where the germs were first developed and other places would soon scatter them over areas more or less considerable. During the siege of Paris small-pox germs largely multiplied in that city, because, owing to privation and depressing influences of every kind, the population were rendered peculiarly susceptible to the influence of zymotic diseases. The stock of small-pox germs accumulated in Paris during the siege has since, there is little doubt, been distributed over a large part of Europe.

In some epiphytic diseases we find the analogues of epidemic and epizootic maladies. The "blights" in the cereals and other plants are caused by the ravages of minute parasitical fungi. A common disease of wheat grain is occasioned by the presence of the fungus *Ureda caries*, the seeds, or sporules of which are so minute that, according to Bauer, a single grain of wheat may contain 4,000,000 of them. The fungi which produce the diseases of plants do not originate sporadically, nor are they ever found except as parasites. For years a whole locality may be absolutely or comparatively free from them, when suddenly those pests will appear and destroy whole crops. It is the same with respect to the ravages of plants by insects: suddenly the caterpillars of moths will appear in vast numbers in localities where they had previously been very scarce. A few years ago the extensive plantations at Dunsany Castle, County of Meath, became suddenly the abode of myriads of caterpillars, which speedily stripped the barks and leaves of a large proportion of the trees. On investigating the nature of the caterpillars, it was found that they belonged to a rare species of moth, which had never been observed in the locality before, but which occasionally appeared in large numbers in certain districts in England. Amongst the numerous insects which ravage our gardens and fields, it is interesting to note that in the seeds of wheat there is occasionally found an infusorial animalcule, termed *vibrio tritici*. It is, however, very many times larger than the vibriones above referred to.

Phytologists acknowledge that they cannot account for the sudden appearance of vast numbers of epiphytic fungi and other pests of the higher members of the vegetable kingdom; but their ignorance in that respect in no way detracts from the positive knowledge which they possess relative to these fungi and insects being the actual cause of epiphytic diseases. Unsuitable soils, excessive damp, and other causes predispose certain plants to succumb to the attack of parasites, but the germs must be at hand, for there is absolutely not the slightest evidence to prove that any of these fungi originate spontaneously. No matter how sickly a wheat plant may be it could not suffer from the blight unless there are fungi to prey upon it.

That which is true of what we may term epidemics amongst plants also holds good with respect to epidemics amongst animals. We do not as yet certainly know, though we may venture on hypotheses, why the germs of disease long absent from a locality may reappear, and fructify to an extraordinary degree. But our want of information on this point is not the slightest obstacle in the way of our belief in the "germ theory" of zymotic diseases. On the contrary, to abandon this theory would be simply to reject the only reasonable explanation as yet advanced as to the means by which contagion is propagated and maintained. If we give up this theory we lay ourselves open to the charge of being believers in the doctrine of spontaneous generation.

If species of bacteria or similar objects are the contagia of certain diseases, then we can understand why it is that so many persons who are near small-pox and fever patients escape, whilst persons not in contact with the infected catch the disease. The bacteria thrown off from the bodies of the sick are not equally diffused throughout the air as a gas or vapour would be, but, for the most part, are scattered about on the clothes and on other solid surfaces, from which they may be conveyed to great distances without making their entry into the body of any one. Ordinary bacteria are not found floating about in the air: if proper precautions are taken an animal liquid may be exposed for months to the air and yet be found free from bacteria; whilst on the other hand if the liquid be allowed to come into contact with a wine glass, or a wall, or ordinary water (unless the latter has been heated to a point at which animal life is impossible), it will soon teem with bacteria. Contagion in general is conveyed by means of clothes or other solid substances, and is rarely directly propagated through the air. In the Report on Yellow Fever, by J. C. Nott, which appears in the Report of the Board of Health of the City of New York, for the year 1870-71, page 388. that writer says:—

"No evidence, I think, could be more complete to establish the probability of a disease. All facts being opposed to its contagiousness, I can come to but one conclusion, viz., that the germ may be closed up in trunks or boxes, or be shut up in the baggage car of a railroad, transported from one point to another (as from Mobile to Grove Hill and Citronelle), and turned loose to propagate and do its work of destruction. The disease was equally fatal at Citronelle and Spring Hill. Contagionists will doubtless regard this as a case of communication by contagion, but from the facts that I have never seen any body communicate the disease, where luggage was not taken with the patient, and that the disease generally goes everywhere that steamboats go from our infected ports in epidemic years, I see no other conclusion than the one I have before given,

viz., that the germ is carried closed up with baggage, and not generated and communicated by personal contagion."

Dr. Nott believes that the germs of yellow fever are not bred within the bodies of men; but still men carry about these germs in their clothes just in the same way that the nurse from a fever hospital conveys contagion from the sick to the healthy, without herself becoming affected with the disease. My friend, Professor Moore, has favoured me with the following letter, the facts contained in which show how the first cases of zymotics in towns originate:—

"WEXFORD INFIRMARY, *March 5, 1871.*

"MY DEAR SIR,—A railway labourer, from Glasgow, came to Wexford on or about the 30th December, 1870; sickened on the 2nd January, and died of small-pox on the 12th. In the house he died, a case appeared early in February, and in the same locality we had over twenty cases, and in other parts of the town say ten or twelve. I have had two cases in private, modified and mild. We have had five deaths, and no new case repeated since the 24th of February.—Yours faithfully,

"H. H. BOXWELL.

"Dr. Moore, King's Professor of Medicine."

There were no cases of small-pox in any of the southern counties of Ireland, in December, 1870; but the disease was epidemic in Glasgow at that time.

Before the question of bacteria as a cause of zymotics arose, Haygarth, Murchison, Ryan of Lyons, and others denied that small-pox poison was directly transmitted through any considerable space in the open air. Murchison asserts that the poison was not contagious in the open air at a distance of half a yard. Chauveau states that the contagious matter of small-pox is volatile—that the solid particles float into the air at a temperature of 40° centigrade, but in his experiments the matter was carried away by a current of vapour. The pent-up gases in a sewer when they find an outlet into a house undoubtedly often carry up mechanically the *materies morbi* whatever it may be, of enteric fever.

It would appear that whilst minute plants and spores float about abundantly in the atmosphere minute animals and ova adhere to walls and other solids. Now, in ordinary disinfection the principal object is to act upon the atmosphere. A bad odour is observed in the sick room wherein lies a small-pox patient, but that odour is not actually connected with the cause of the disease: the contagious matter which produces small-pox is odourless. We might destroy the bad odour in the room without diminishing the quantity of contagious matter present.

The following experimental results prove that the ordinary disinfection

by gases does not kill the bacteria which are usually associated with putrefaction, nor does it perfectly destroy the contagious matter of at least one zymotic disease :—

Several watch and microscopic object glasses were dipped into filtered beef-tea, which contained enormous numbers of bacteria, and whilst still moist they were placed in different positions in a hood, or small chamber made of wood and glass, containing $16\frac{1}{2}$ cubic feet of space. Half an ounce of chloride of lime was placed in a capsule and introduced into the hood, and an equivalent quantity of hydrochloric acid was poured on the powder in such a way as to prevent the chlorine evolved from passing out of the hood. After the lapse of 24 hours the door of the hood was opened and the glasses removed. They were found to be covered with extremely thin films of solidified beef extract. A few drops of pure water were used to render the film semi-liquid, and its contents were examined with a microscopic power of 800 diameter. In a few seconds the bacteria were detected moving about with great rapidity, and with apparently undiminished vigour. It was clear then that fumigation at the rate of a little more than three ounces of bleaching powder per 100 cubic feet of space had no effect in destroying bacteria. The hood was more air-tight than a room is when its doors, windows, and fire-place are closed. On opening the door of the hood after 24 hours the odour of chlorine was distinctly perceived at a distance of several feet. If a room 15 feet long, 10 feet wide, and 10 feet high, and having, therefore, a capacity of 1,500 cubic feet, were disinfected by chloride of lime in the relative proportions employed in the foregoing experiment, it would be necessary to use nearly three pounds of chloride of lime.

As the gases evolved from three ounces of bleaching powder per 100 cubic feet of space did not destroy bacteria, an experiment was made with one ounce per $16\frac{1}{2}$ cubic feet, or at the rate of nearly six pounds per 1,500 cubic feet—the size of a small room. The result was similar to that of the first experiment, the bacteria being almost as lively after as they were before the process.

The next quantity tried was one and a half ounces of bleaching powder per $16\frac{1}{2}$ cubic feet, or at the rate of about eight and a half pounds per a room of 1,500 cubic feet capacity. After 24 hours exposure to the highly chlorinated atmosphere produced by this experiment the greater number of bacteria were not only alive, but most of them exhibited the utmost vitality. Two ounces of bleaching powder per $16\frac{1}{2}$ cubic feet were next tried, and after exposure to the gases evolved from this quantity by the action of an acid, the bacteria, though languid, were still mostly alive, and a few of them were very active. The last experiment was made with three ounces of bleaching powder per $16\frac{1}{2}$ cubic feet, or at the rate of $16\frac{1}{2}$ pounds of the disinfectant to 1,500 cubic feet; but even the enormous

amount of gas evolved from this quantity failed to kill the greater number of the bacteria subjected to its influence. Films of moist meat extract containing bacteria were next exposed to an atmosphere of equal parts of chlorine gas and ordinary air. This operation was conducted on an air-tight glass vessel. After 24 hours they were examined, and no life could be perceived. Dried films of meat extract containing bacteria were submitted to the influence of this gaseous matter, but after being moistened many of the bacteria were found still alive though almost inactive.

Similar experiments were made to ascertain the action of sulphurous acid gas upon bacteria, but this gas was also found to produce but little effect on these animalcules.

Having proved that the bacteria, which exist in liquids, are not destroyed by exposure to atmospheres highly charged with chlorine, or sulphurous acid gas, an experiment was next made with the view of ascertaining whether or not the germinal matter of bacteria could be destroyed by ordinary gaseous disinfection. Accordingly filtered beef tea, which did not exhibit any forms of life under the microscope, was divided into three parts. One portion (*a*) was placed in an ordinary test tube, and loosely plugged with cotton wool; another portion (*b*) was placed in a test tube, which had shortly before been heated nearly to redness, and loosely plugged with cotton wool, which had been highly heated to 350° ; the third portion (*c*) was poured upon microscopic objects and shallow watch glasses, and these were placed in the hood and exposed for 48 hours to the gases evolved from two ounces of bleaching powder, treated with acid. In 24 hours the liquid (*d*) which had simply been deposited in a test tube, was found to swarm with mycrozymes; the liquid which had been placed in the tube that had been heated to redness was, after a week, found to be free from animal life,^a whilst a few hours after their removal from the hood, the films of beef-tea exhibited swarms of vibriones, although every precaution was taken to prevent contamination from solid surfaces subsequent to the removal of the glasses from the hood.

The following experiment was made in a room of 1,600 cubic feet capacity. Animal liquids containing microzymes were placed upon the chimney-piece, upon the window panes, and other smooth parts of the apartment. Seven pounds of chloride of lime were then decomposed by oil of vitriol, and the room carefully closed up. After 48 hours the room was opened, and the films containing the bacteria were, with every proper precaution, washed upon object glasses, and examined

^a Saunderson has shown that animal liquids, deposited while fresh in tubes which had been highly heated, and loosely plugged with cotton wool, remained free from bacteria for a long time.

microscopically. In every case there were large numbers of living microzymes.

The next experiment was conducted as follows:—

Four ivory points, charged with vaccine lymph, were placed in the hood, and subjected, during 24 hours, to the influence of the gases evolved from one ounce of bleaching powder, decomposed by acid. With these points I successfully re-vaccinated two persons. The other points failed. Six charged points were next exposed to the gases evolved from two ounces of chloride of lime per $16\frac{1}{2}$ cubic feet; but attempts to vaccinate with these points proved unsuccessful.

The results of these experiments show that bacteria and the contagious particles of vaccine lymph resist, when protected by an extremely thin film of solid, or semi-solid matter, the action of chlorine and sulphurous acid gases applied to them in larger quantities than are usually employed in disinfection. The filtered meat-juice used in these experiments contained only five grains of solid matter, per ounce of 480 grains—less than one per cent. The object glasses were dipped in this liquid, and many of them allowed to drain before being subjected to experiment. We may readily conceive then how extremely thin the film was that separated the bacteria from the gases set free in the hood. It is extremely improbable that the actual contagious particles of small-pox or cholera, or similar diseases, are ever detached from the serum and other matter with which they associated when thrown off from the body. They are, no doubt, invested with some such film as that which protects the contagious granules in vaccine. If ordinary gaseous disinfection sometimes fails to destroy the vitality of vaccine, and has no effect on ordinary microzymes, we cannot rely upon it as a means of destroying the contagiums of zymotic diseases which certainly are near akin, if not to bacteria, at least to the virus of vaccine. The recent experiments of Grace Calvert, show that bacteria sustain a very high temperature without being killed; and on the other hand, Melsens in the *Journal de Pharmacie et de Chimie* for September, 1870, shows that vaccine lymph retains its activity when exposed to the intense cold of 80° centigrade. The low forms of life are often capable of resisting influences which, in the case of the most highly organized animals, would produce fatal results.

No doubt, chlorine, sulphurous acid, and some others of the so-called disinfectants, destroy bacteria and contagia; but in order to do this, they must be employed in much larger quantities than they have hitherto been used. *Strong* solutions of disinfectants, when mixed with liquids containing microzymes, kill these animalcules, and the germinal matter from which they are evolved; but gases appear to have comparatively little effect in destroying bacterium life.

The following directions for disinfection, issued to the medical officers of the army, will serve to illustrate the absurdity of the present methods of disinfection :—

“To fumigate a room, 60 feet long, 20 wide, and 12 high,

1. Take common salt,	-	-	4 ounces.
„ oxide of manganese,	-	-	1 „
„ sulphuric acid	-	-	1 „
„ water	-	-	2 „

The water and acid to be mixed together, and then poured over the ingredients, in a delft basin, which should be placed in a pipkin of hot sand. With nitrous acid gas,

2. Take copper shavings,	-	-	$\frac{1}{2}$ ounce.
„ nitric acid	-	-	$1\frac{1}{2}$ „
„ water	-	-	$1\frac{1}{2}$ „

Pour the acid and water upon the copper, in a small jar. With sulphurous acid gas,

Burn two ounces of sulphur in a pipkin.”

The complete disinfection of a room tainted with the poison of contagious disease can only be accomplished by the most thorough cleansing. The paper should be removed from the walls, and the latter scraped. The ceiling should be washed and whitewashed, the woodwork and floors should be scoured—all these detergent processes remove—probably without utterly destroying them—the contagious particles. The old-fashioned plan of simply whitewashing the walls and ceiling of a room, and washing the woodwork has much to commend it, and it is infinitely more efficacious than gaseous disinfection without liquid applications. If the whitewash does not kill the bacteria, it certainly imprisons them securely. The disinfection of the air of the room is best accomplished by a solution of chloride of lime, carbolic acid, chromic acid, chloralum, &c., applied in the form of spray; but this process is not likely to be generally adopted. A little chlorine may be generated in the room, and if it do no more than remove a bad odour, it will prove useful. As people cannot comfortably breathe in a room which has just been disinfected by sulphurous acid, or chlorine, they are obliged to open doors and windows, in order to admit the fresh air. In this way the use of disinfectants is to be commended, because it obliges people to ventilate their apartments. If solutions of disinfectants be applied to the walls and woodwork, they should be strong ones—say half a pound of chloride of lime to an imperial gallon of water. With respect to clothing and furniture, unless they can be treated with strong disinfecting solutions, or exposed to a temperature of 320° Fahrenheit for 8 hours, it were better to destroy them by fire.

DR. MALACHI BURKE thought Dr. Cameron's paper one of great importance. Being a member of the Small-pox Relief Committee, and Dr. Cameron being connected with the Public Health Committee of the Corporation, he wished to refer to what had been recently stated by a distinguished member of that Committee—at least he supposed he was distinguished because he was an alderman. That gentleman came to the Mansion House a few days ago and said a house or a room could be disinfected in two or three hours. If the work of disinfection could be done so rapidly, what were they to say to Dr. Cameron's paper? He wished to ask Dr. Cameron what time he would consider necessary for the disinfection of a house?

DR. GRIMSHAW said the same distinguished gentleman referred to by Dr. Burke stated that the Corporation disinfected clothing by heat in two hours, but Dr. Cameron said it would take at least eight to do it effectually. The process of disinfection was carried out by the officers of the Public Health Committee after this fashion. A man comes with a pint of chloride of lime in an old battered tin; he dilutes it with sulphuric acid, places it in a vessel in the middle of the room, shuts it up and leaves it there until the next morning. The usual course when he turned his back was for the owner of the room to throw the disinfectant out of the window. There were but two men at present employed by the Public Health Committee disinfecting the whole of Dublin, so that chemical disinfection, or disinfection by gas, was not fairly tested in this city; and it appeared from a statement published by Mr. John Norwood in the public journals, that only one in 35 of all the houses reported as being infected by small-pox had been disinfected. The whole question that Dr. Cameron had discussed depended on a point which was by no means sufficiently determined; he did not believe it was proved that bacteria were the essential accompaniments of contagious disease, so that the killing or non-killing of those organisms could not be taken as a proof that disinfection had been effectually accomplished or not. Of course it was an important matter that chlorine gas would not kill certain things which were looked upon as organized beings, but Dr. Cameron added that these were the necessary accompaniments of contagious disease. He (Dr. Grimshaw) did not think they had any proof at present that such was the case, and the destruction or non-destruction of the bacteria did not settle the matter one way or the other. So far as they knew at present, the only effective disinfectant was a high heat, and even more important were detergent measures. It was only recently it had been announced in Dublin that gas was a disinfectant. He found, on looking over the records of Cork-street Hospital, that

formerly such a thing was never attempted as trying to purify a house by fumigation. The houses in which there had been contagious disease were all cleaned, scraped, and swept out. This was the plan pursued until the matter was taken up by the city authorities, and they seemed to have assumed that a small quantity of chlorine spread in a room would effect its purification.

DR. DARBY said he was reminded by that discussion of Mrs. Glassis' directions in her cookery book for making hare soup—"First catch your hare." They were talking about contagion, and infection, and disinfection, but they were by no means agreed as to what contagion was. He admitted that Dr. Cameron's paper was a valuable one, because it went to undeceive people who thought they were doing a great deal of good, whereas they were doing none at all. They did not know what was the cause of epidemic disease. Was it those little animals referred to? It was not at all proved they were the cause of disease, and he did not believe they were. He had come to the conclusion that there was scarcely any disease which affected the human body that might not at one time or other become epidemic. He had been for over 30 years connected with a large public institution, and he had not the slightest doubt from his experience there that vermin were sometimes epidemic; that itch was epidemic; that chronic purpura was epidemic. At one time itch broke out and every one in the institution would have it. At another time there would be a plague of lice; whilst on other occasions very few of the inmates would be thus affected. Individuals might come into the house in a lousy condition, and when cleansed there was no more of it; while at another time every one would be affected with these parasites, and the nurses would have the greatest difficulty in keeping the inmates clean. He had had thirty cases of chronic purpura in the hospital at one time after the famine. It was supposed by some that disease was propagated by a germ, but he did not find any satisfactory proof of that proposition. Cerebro-spinal arachnitis was not known in Ireland until it appeared in his hospital. It remained for the late Dr. Mayne to throw more light on the disease than he could. All he could do was to bring case after case forward at the Surgical Society in order to elicit information, but not a man in the room knew anything about it, and many of them thought he had found out a mare's nest. It appeared among healthy boys in the workhouse school, and it attacked none but boys of from 10 to 15 years of age. It was next seen in Belfast, and then it came to Dublin. Take the present epidemic of small-pox. I went about in the same way.

DR. BURKE rose to order, and submitted that Dr. Darby was not speaking to the question.

DR. DARBY said he must establish a premise before he could draw a conclusion. He proceeded to say that when the small-pox appeared last year it showed itself in Dublin, in Kingstown, and in Wicklow, and it hopped over all the intervening spaces. That did not seem as if the disease were communicated from one person to another. The question was a serious one in an economic and medical point of view. In the process of disinfection now employed they were using chemical re-agents to destroy they did not know what. He believed that plenty of soap and water and pure air were the best disinfectants. When small-pox appeared in the hospital under his care he placed over the doors of the ward a curtain steeped in a solution of chlorate of lime. He did not know that it did any harm, and he doubted whether it did any good; but he had great faith in fresh air and in cleanliness. The fact that these epidemics come suddenly, rise gradually to a maximum, and then decline was an argument against the germ theory. If that theory were true would not the epidemic continue to spread more extensively the greater number of people suffering from the disease, and, therefore, the greater the likelihood of its being communicated to others. The conditions of the country were constant, the conditions of epidemics were not constant—they were variable, and at present but little was known on the subject.

DR. FINNY should have hesitated to make any remarks on the subject were it not that he thought the two last speakers seemed to have misunderstood what Dr. Cameron meant to suggest. Dr. Cameron did not state that bacteria and fever germs were identical, but he assumed that the fever germs were animal or vegetable substances, certain small molecules, each propagating its own specific disease, and no other—the virus of small-pox giving small-pox, that of scarlatina scarlet fever, and that of vaccine cow-pock. He, secondly, assumed that the bacteria, or vibriones, or microzymes are found in putrescent matters, and also wherever there is any disease that appears to be spreading from one individual to another. He did not, however, assume that the bacteria were identical with the germs of the disease. The third point made by Dr. Cameron was, that the ordinary gaseous disinfectants had no effect in destroying the vitality of these bacteria, and, therefore, he assumed, and with much reason assumed, that the same disinfectants, similarly employed, had but little power in destroying the ordinary fever germs. He said that heat only had been found adequate to destroy these bacteria, and, therefore, he assumed that it was likely to be the true and only way of destroying fever germs also. He (Dr. Finny) thought the question one of immense importance, and while Dr. Cameron's able paper showed how little useful their efforts had been heretofore in destroying these

germs by gaseous disinfectants, it also showed how that by proper detergent measures, and by applying great heat to such things as would stand it, they had in their hands a power of limiting to a certain extent these poisons, and thus checking the spread of contagious diseases.

DR. LYONS rose to say a few words, rather to avoid being taken as concurring with many of the statements that had fallen from Dr. Cameron than as thinking they could enter profitably on the present occasion into a discussion of the many points he had raised. The basis of his paper seemed to be an assumption that the germs for the introduction of disease were different for different diseases, and that, in the next place, those germs or microzymes had some close affinity, or relation to, if they were not identical with bacteria, vibriones, &c. He (Dr. Lyons) could not be taken as concurring in the idea that minute microscopic animalculæ or vegetations were the germs or origin of any but a few and well defined forms of cutaneous disease. He thought they were travelling in an entirely wrong direction in looking for the origin of epidemic disease in that quarter. He had himself an idea that diseased processes, however variable in their superficial manifestations, were, when viewed from a profound pathological point of view, more nearly allied, and much more closely similar in their essential conditions than would be supposed from a superficial view of them. He had in his own mind the elements of a theory of disease, of which he was not yet prepared to produce the proofs in all their details, the principle of which was the unity of all diseased action in the body: that whereas, for instance, in cases of cerebro-spinal meningitis there was an especial manifestation of nervous power of one kind. There were other forms of disease, in which the activity of the nervous system was brought into play in a different manner, causing one class of eruption on the skin in one epidemic, and another kind of eruption in another epidemic. But that was a subject of too wide a range to be followed upon that occasion. He did not think Dr. Cameron had undertaken to say that the use of gaseous disinfectants would not to some extent destroy the supposed germs of disease, admitting, for argument's sake, that such germs did exist. What his paper had shown, as far as it had shown anything, was that by the use of very large quantities, and especially by potent solutions, of certain powerful agents, we could destroy the vitality of bacteria, or prevent their development. He (Dr. Lyons) for one was not prepared to admit that all diseases were propagated by contagion, that like generated like in disease, or that contagion would explain the development of all epidemic diseases. He doubted if, in the present condition of knowledge, any two persons in that room would agree as to what was, and what was not, contagion, or what was to be

accepted as the element of contagion in any given case, or, if admitted, how its action was to be explained.

He might observe that he thought they were all too hard on the Corporation of Dublin. How could that body undertake for the disinfection of a large city, to employ the thousands of tons of disinfecting agents which would be necessary on the basis of Dr. Cameron's calculation? He admitted that some very absurdly inadequate processes had been employed to disinfect the sewers and the Liffey; but under the circumstances the Corporation was perhaps not so much to blame as some appeared to think. In his opinion they were a long way off yet from having ascertained the true origin of any of the principal forms of epidemic diseases. His view was that they followed a certain course of development in cycles, and were to be explained by the destruction of a certain portion of nervous energy in the individual and in the population, and not so much as was ordinarily supposed by any form of contagion, in the sense in which that word had been hitherto employed so vaguely, and so unscientifically.

DR. ARTHUR WYNNE FOOT said that he had participated in the pleasure and profit which had been derived from hearing Dr. Cameron's paper. He had only to regret that Dr. Cameron had not had more time for its elaboration at his disposal, for he was certain that in that case he would have developed some points to a further extent, and have removed what might have appeared a little obscure to some who had listened to his communication. The principal topic to which Dr. Cameron had addressed himself—the disinfection of dwellings in which there had been sickness—was inseparably connected, as Dr. Cameron had stated, with that most important question, the nature of contagion. Dr. Foot did not wish to offer any observations upon the subject of the disinfection of dwellings, not having experience in this matter in any way comparable with that possessed by one discharging the important duties which Dr. Cameron did. On the subject of disinfection of the blood he would have made some remarks, but this question had not been raised in the paper just read, and he would comply with the excellent suggestion of their experienced chairman, and strictly confine himself to the points arising out of Dr. Cameron's paper.

Considering that the whole subject of disinfection hinged upon the views entertained of the intimate nature of contagion, Dr. Foot wished to take this opportunity of declaring himself to be a believer in the germ theory of disease. The causes which had led him to adopt those views were various. He had been long engaged in private and public medical teaching, and having always felt it to be his duty to keep the students informed upon the present condition of modern medicine, he found himself, in

consequence, to some degree, acquainted with what had been written upon this very prominent medical question, one which of late years was assuming proportions which must force it upon the attention of every one entering upon the study of medical science. He had also been largely biassed in its favour by looking through the ranks of science, and seeing what kind of men held this doctrine; the result was that while the most advanced minds in the exact science of chemistry, Liebeg and Hoppe-Seyler, and such physicians and physiologists as Beale, Sanderson, Simon, Huxley, Tyndall, were arranged as advocates of this view, he found no such list on the opposite side.

He would also observe that those who repudiated the germ theory must, if they had any opinion at all on the subject, advocate the doctrine of spontaneous generation; they must be either Panspermatists or Heterogynists. The germ theory had been spoken of this evening rather disparagingly, as a vague hypothesis, an assumption, &c., by some who did not appear to have given the matter the consideration it required and deserved; he (Dr. Foot) thought that demonstration was removing this question from the region of hypothesis, and considered it would be more properly spoken of as a deduction from an immense amount of circumstantial evidence, and the result of many converging lines of thought. It offered the most plausible explanation of the strange security from infectious disease that some persons exhibit, of the very opposite condition in others, and of the peculiar phenomena of intermittent fever. While advocating the broadest tolerance of opinion on all great questions of medical science, Dr. Foot considered that in discussing such a subject as the germ theory of disease it was only fair that its opponents should be acquainted with the facts upon which the theory they contested was founded.

The CHAIRMAN thought Dr. Cameron had, to a great extent, established the object he had in view with respect to the action of disinfectants. He had shown that it required a much higher degree of concentration to destroy animalcules than had been heretofore considered necessary. In the course of his observations he alluded to two diseases with which they were familiar, itch and syphilis, as unmistakable examples of contagion. That every one would admit; but they would also admit that there was a wide difference between contagion and epidemic influences. That was an important matter to keep in view. With respect to disinfectants, they all knew how often disinfectants and deodorizers were confounded. As to the germ theory, he was not very well versed in it, but he was not aware of any fact proved that pointed to the existence of particular germs for disease. He thought until they had something more tangible than they had as yet, they were not able to say that disease was propagated

by germs. He thought the study of the natural history of epidemics would throw more light on the subject. They all knew the extraordinary way in which cholera spread itself, the exceptional manner in which it progressed, attacking one side of a street and passing by the other. In the last epidemic of variola, which he had an opportunity of seeing, he made special inquiry of the patients who came into Dun's Hospital, and not one of them could say how he contracted the disease, and in the majority of cases there was no case of small-pox in the vicinity of their residence. The Metropolitan Sanitary Board of London requested returns to be made from all the vestries who employed people in cleaning the sewers, of the number of persons employed in that way, the age of those persons, the duration of their employment, and the cases of fever amongst them. 234 individuals were included in the returns. A great many of them had been engaged 17 years in that occupation, and the whole sum of the cases of fever amongst them was six. That struck him as a very remarkable event.

DR. CAMERON, in reply, said that Dr. Finny had so clearly put his views before the meeting that he found it unnecessary to recapitulate them. All he could find in Dr. Grimshaw's remarks was that there was a something in contagion which heat destroyed. If that something were any of those abnormal conditions of the air, or loss of nervous energy, of which they had heard, he did not see how an increase of temperature or whitewashing could destroy it. There was a something which was capable of communicating disease from one individual to another; for there could be no doubt there was contagious matter in small-pox, pustules in farcy in the horse, in pleuro-pneumonia in the ox, and in vaccine, and we could produce any of those diseases by introducing a certain kind of morbid matter into the blood of an animal.

The CHAIRMAN.—You are now speaking of palpable matter.

DR. CAMERON observed that this palpable matter could be reduced in an almost infinitesimal degree, and yet be capable of conveying disease. Any one who read Chauveau's experiments would see he clearly proved there were living particles in vaccine, and in the matter from sheep and ox pustules; if they were taken out of the fluid it became non-contagious; but if left in it it remained contagious. What could be a greater proof of the germ theory? Dr. Darby stated that lice assumed an epidemic form. Did not that go far to prove the germ theory? Surely Dr. Darby did not mean to say that where lice and itch became epidemic diseases there was an actual creation of lice and acari? If he did not go to that extent, he must admit that those diseases could only be propagated from individual to individual. With

regard to these diseases, they might be spread in a great many ways which appeared to be extraordinary and unaccountable; but when they carefully inquired into the mode by which the contagion was communicated, they were often able to penetrate the mystery. For example, the recent report of the Board of Health, in Victoria, gave an extraordinary instance of the spread of small-pox contagion by myriads of flies in a place where there was little or no communication with the districts in which the disease first appeared. It was found that the disease spread in the direction of the flight of myriads of flies that had previously tormented small-pox patients, and which carried from them the contagious matter, whatever it might be. Dr. Lyons, he knew, was not a strong contagionist, but he misunderstood one or two of his (Dr. Cameron's) experiments. He did not mean to say chlorine would not do some service. All he wanted to say was that unless used in very large quantities it did not destroy the lowest forms of life, and then he asked why did they use chlorine at all? Every physician who used chlorine must have had some foregone conclusion as to its effect? Surely it was not to produce a change in nervous energy; it must be to do some specific thing—that is to destroy contagion—and could anything be more reasonable if contagion was to be found in the floating objects in a room? The germs of disease, or, at all events, bacteria, did not float in the room, and they were not destroyed by the quantities of the disinfectants ordinarily used. To destroy them larger quantities must be employed, and they should be, in great part, used in solution. He had dipped a brush in a strong solution of chloride of lime, and passed it over a glass on which there were bacteria. He then passed over this water, which had been previously heated to a high degree, and he found the bacteria were destroyed. With regard to syphilis being an epidemic, the first time it was heard of it assumed something of that character.

The only reason why syphilis did not become an epidemic was that its poison was not disseminated through the air as easily as other kinds, it was more ponderous. Dr. Russell, of Glasgow, a very accurate observer, showed that all the persons connected with the Fever Hospital of that city, the ward-maids, the nurses, the store-keeper, the man at the gate, sooner or later caught fever. There were different degrees of rapidity with which the poison of contagion was propagated. Actual contact was required to give syphilis, whilst small-pox, being more volatile, might be carried in minute particles in the clothes. As to what Dr. Smith had said about sewers, it establishes the theory of contagion. There was no other theory that afforded a satisfactory solution to immunity from epidemic diseases in certain cases. The immunity of the persons employed in cleaning the sewers merely showed that decomposing ordinary animal and vegetable matter *per se* would not produce zymotic disease. He

knew a family in the country who persistently drank water that contained 20 grains of organic matter per gallon ; it had even a bad odour, and it came from a well so situated that the drainage from the stable-yard and out-offices flowed into the shaft of the well. The family, as he had said, drank it continuously, and yet no contagious disease had ever broken out amongst them. Why? The water was impure, it had a bad smell, but the germs of disease were not there. Time was a great factor in disinfection. He found that the quantity of disinfectant matter applied in gaseous form which would not kill bacteria in a short time, would do so after a prolonged contact. They could not properly disinfect a house in less than 24 hours. No house in which a small-pox patient had died could be considered free from contagion until the walls were scraped and white-washed, and the place thoroughly swept out.

The Society then adjourned.

[SPECIAL MEETING.]

Wednesday, May 8th, 1872.

DR. FITZPATRICK, Vice-President, in the Chair.

After the minutes had been read, DR. LYONS said :—

Sir,—A resolution has been placed in my hands since I came into the room, which I rise to propose under no ordinary feelings. It must be in the recollection of us all that a gentleman occupied the chair at our last meeting who has since been called from amongst us. Apparently in the vigour of his green old age, and giving promise of being with us for many a year to come, to aid us in the discussion of medical subjects by his great and varied experience, Dr. Beatty presided, on that occasion, with the dignity, blended with kindness of manner, which lent a grace and a charm to everything he took part in. He has been unexpectedly called from amongst us, and to no one who had the advantage of his acquaintance has this event brought more deep feelings of regret than to myself. Since my entrance into this Association, and since his election into the College of Physicians, Dr. Beatty and I have been associated together in many important acts. We have likewise often stood opposed to each other, and there is no way in which men learn to respect each other more than when they bring to discussion opinions conscientiously arrived at from different points of view. Sir, it was a pleasure to meet Dr. Beatty in argument in the College of Physicians or in this Association, and to differ from him was but to call forth the best qualities of his

well trained intellect, of his cultivated mind, and of his generous heart. I think, Sir, that in this Association, where almost everyone may claim to have had the honour of an acquaintance with Dr. Beatty, it would be quite superfluous on my part to enter into any lengthened eulogium of him. His memory will ever live fresh in the Halls of this College. I therefore, Sir, with deep feelings, conclude by moving the following resolution:—"That the Society takes this opportunity to express its regret at the death of Dr. Beatty, who so frequently presided at meetings of this Association, and that it begs to express its sympathy with his widow and family."

The resolution was seconded by Professor MOORE, and passed unanimously.

DR. HAYDEN, in opening the discussion on Dr. Lyons' paper on small-pox, said:—

It would be manifestly impossible to discuss, within the limited period ordinarily allowed to each speaker by the rules of the Society, a paper of the length and exhaustive character of Dr. Lyons' in such a manner as to do justice to its great merits, or to be satisfactory to the speaker himself. I must, therefore, throw myself on the indulgence of the Society, and crave an extension of time.

In my remarks I shall endeavour to follow the order, as the most convenient, in which the several topics are discussed in Dr. Lyons' paper, and that which first calls for notice is the low mean mortality of 11 per cent. presented by the returns of the House of Industry Hospitals for the decade 1861–71, as contrasted with that of the present epidemic in the same institution for the years 1871–72, as reported in Dr. Lyons' paper, namely, 19·47 per cent. I am quite in agreement with Dr. Lyons when he states, that for the successful invasion of epidemic disease, a state of "receptive capacity" in those attacked is no less essential than the operation of the morbid agent itself, and I think he has furnished a striking example in proof in the case of the Swedish sailor mentioned in his report.

The first patient admitted into the Hardwicke with small-pox in the present epidemic was, I find, Anne Cox; she was admitted on the 28th of February, 1871, and no person took the disease from her. On the 3rd of February in the same year, and therefore twenty-five days previously, the first small-pox patient (Eliza Delany) was received into the Mater Misericordiæ Hospital, and from her no less than seven persons took the disease, including students, sisters, wardmaids, and patients with whom she came in contact during convalescence. It seems to me difficult to account for the absence of "receptive capacity" for the disease on the part of those exposed, at the outset of so formidable and wide-spread

an epidemic as the present, which the history of Anne Cox's illness presents.

I may be permitted to introduce here in summary the small-pox statistics of the Mater Misericordiæ Hospital, because I think that, placed in juxtaposition with those of the Hardwicke Hospital, as furnished by Dr. Lyons, they are calculated to emphasize many of the instructive and useful truths which he has elicited.

From the Hardwicke Hospital I find the vaccinated cases returned amount to 541 out of a total of 611, with a mortality of 11·82 per cent. In 3 cases the patients had been re-vaccinated, and all three recovered; 66 were unvaccinated, and, as I infer, likewise unprotected by inoculation, or by a previous attack of small-pox; amongst these the per centage mortality amounted to 78·57.

Of the typical purpuric form of small-pox there were 21 examples, and all were fatal but one.

The total small-pox admissions into the Mater Misericordiæ Hospital from the 3rd of February, 1871, to the 30th of April, 1872, both inclusive, amounted to 429. Out of this number there were 73 deaths, or a total mortality of 17·01 per cent. The vaccinated cases amounted to 320, with 35 deaths, or a per centage mortality of 10·93. In four instances the patients had been re-vaccinated, and in all four the disease was fatal. With regard to these four cases the following particulars may be interesting:—A boy, aged ten years, was re-vaccinated two months before taking small-pox, which he had in the confluent form, and died. The other three patients were adults, and had been re-vaccinated four years, seven years, and eight years respectively, before taking small-pox, which in all was fatal. Unprotected by vaccination, inoculation, or a previous attack of small-pox, 71, of whom 39 died, representing a mortality of 54·93 per cent.

The purpuric and hæmorrhagic cases were 38, and in every one of these death was the result. These cases belonged to category No. 3 in Dr. Lyons' report, and were characterized by the following symptoms:—great prostration at the period of invasion; the appearance of purple spots thickly studded on the chest and hypogastrium about the third day, and on the fourth, a boiled-lobster discolouration of the entire cutaneous surface of the body, interspersed with a few dark purple puncta; the conjunctivæ were deeply injected and slightly ecchymosed, as were likewise the palate and pharynx; the tongue was somewhat swollen, indented at the edges, moist, and covered with a white cream-like fur; and hæmorrhage from the bladder or lungs, or from both, appeared in small quantity.

On the next day (the fifth), the surface was of a darker tint; the face was swollen usually; and hæmorrhage was more copious. On the sixth

day the colour of the skin was decidedly purple, and on the forehead, chest, abdomen, and legs, large black spots, more or less rounded, about the size of a split pea, not raised above the surface, and as if caused by the dropping of slightly dilute ink, were visible. These spots I have been in the habit of designating *maculæ lethales*, from the circumstance that death has invariably, and, within the succeeding twenty-four hours, followed their appearance. The entire conjunctivæ were, at this period, of a dark purple colour, and elevated round the cornea to a height of two or three lines, the pupils contracted, and the irides of their natural colour, and contrasting, in the most striking manner, with the surrounding conjunctivæ. The expression of the eye was singularly peering, and never to be forgotten. In one instance I have seen large blebs of extravasated blood on the palate and inside of the cheeks. Hæmorrhage is now copious, and, usually, on the seventh day death has taken place suddenly, without a struggle, and apparently by failure of the heart.

It is noteworthy that the pulse in these genuine purpuric cases rarely exceeds 90, and is usually not more than 84 in the minute; respiration is unembarrassed; the temperature is not much, if at all, above the normal standard; the power of deglutition is unimpaired; suffering is very trivial; and consciousness is perfect up to the moment of death. Furthermore, these cases are characterized by the total absence of pustules, a few papules being the only representatives of the variolous eruption. I entirely subscribe Dr. Lyons' statement that the resemblance of this form of small-pox to what we had such sad experience of a few years ago in Dublin, under the name of "purpuric fever," is so close, that had it not been presented to us in an epidemic of small-pox, and in many instances associated, but in a minor degree, with small-pox pustules, it would have been set down as belonging to the latter disease. I am likewise in agreement with Dr. Lyons, when he declares that the mortality increases in a direct ratio with the age of those affected, with only a single reservation, namely, that, according to my experience, it is greatest in early and unprotected infancy.

It is remarkable that whilst at the Hardwicke, the hospital devoted to the reception and treatment of small-pox patients, only one of the servants of the institution took the disease; no less than five nurses and wardmaids in the Whitworth Hospital, which is a detached building, separated from the former by a broad roadway, and not open to those stricken with small-pox or other acute febrile disease, should have contracted it. I should like to know whether those in attendance on the sick in the Hardwicke had been specially fortified against the invasion of small-pox by re-vaccination, or by a recent attack of the disease itself; and, in reference to the number of persons attacked in the Whitworth, whether there had been any restriction on the intercourse between the two

hospitals, through nurses, wardmaids, and students? This inquiry has been suggested by the great number of ordinary patients attacked by small-pox in the Hardwicke, namely, 44; whilst in the Whitworth only 14 were affected. In the Mater Misericordiæ Hospital during the year 1871, when, owing to an inadequate appreciation of the extreme contagiousness of small-pox, free communication between the small-pox wards and the other portions of the hospital was permitted, as in the case of the ordinary fever wards, through students, sisters, and wardmaids, and, I must add, when the rule now in operation amongst the medical officers of the hospital, of making their *last* visit to the small-pox wards, was not observed, and those in attendance on the sick were not protected by re-vaccination, or by a recent attack of small-pox, no less than *thirty-one cases* of small-pox, contracted in the hospital, occurred, including students, sisters, wardmaids, and ordinary patients. Of these, seven, as already stated, were directly traceable to the first patient admitted. During the present year (1872) greater caution has been observed; all direct communication has been intercepted. A qualified resident medical officer has been appointed to attend exclusively to the small-pox patients, and is not allowed to visit the other wards; a distinct administration has been provided for the small-pox wards, and the resident pupils of the hospital are not permitted to enter them. The result of these precautions, I am happy to say, has been, that during the current year, when, up to the 30th April, 344 cases have been admitted, only *two* cases of the disease contracted in the hospital have occurred. Out of this total of 344 patients admitted within the current year, 64 have died, representing a mortality of 18·6 per cent., and indicating the more formidable type assumed by the epidemic in the second as compared with the first year of its operation.

I am astonished to find Dr. Lyons sceptical as to the alleged instances of communication of small-pox through clothing and furniture, which he characterizes as “stories.” In the case of the first patient received into the Mater Misericordiæ Hospital, the girl Delany, the disease as already stated was undoubtedly contracted in that way; and in another instance in a neighbouring locality with which Dr. Darby is familiar, a laundress took small-pox from washing the clothes of a person who had recently had it, and had gone for change of air to the place alluded to. In this town, as I have been informed, not a single case of small-pox had previously occurred. I am a firm believer in the communicability of small-pox, scarlatina, and cholera, through clothing and other *fomites*; and in reference to the last-mentioned of these diseases I have given several examples in proof in my report of the cholera epidemic of 1866.

The purpuric form of the disease, as Dr. Lyons truly remarks, respects neither age, sex, nor previous health or habits. Indeed the great majority of those whom I have seen affected with small-pox of this type

were of adult age, and many of them had enjoyed previous good health, and were of regular and temperate habits. This is a blood poison of the most virulent character. In the course of the present epidemic I have observed at least eight cases of a hybrid form of measles, distinguished from the genuine type of that disease by the absence of pyrexia and catarrh; there has been slight malaise, and on the second or third day, and quite suddenly, a rubeoloid eruption has appeared all over the body, and concurrently the patients experienced complete relief from all unpleasant sensations; the pulse not exceeding 84; the skin cool; appetite and sleep unimpaired, and no cough or coryza.

The eruption, in large patches, with strips of intervening healthy surface, has lasted much beyond the period of ordinary measles, eight or nine days, becoming dusky towards the end of this period, and *not followed by desquamation*. No unpleasant sequelæ of any kind have been presented in connexion with these cases. I have been in the habit, for convenience sake, of designating the affection *rubeola notha*.

The advantage of free and early pustulation has been noticed by Dr. Lyons. I have also observed this; and I am satisfied that short of extreme confluence it is desirable. The worst cases, not purpuric, which I have met, have been those in which the eruption "aborted" about the fourth or fifth day, and never advanced to pustulation. Dr. Lyons has noticed a succession of troublesome boils as a not infrequent sequela; I have observed a similar phenomenon, and likewise multiple abscesses in the neck and axilla, and on the arms and legs; and in nearly all patients in whom they have been presented, there were decided symptoms of the scorbutic diathesis, such as bleeding from the gums, epistaxis,* &c., and an anti-scorbutic treatment has been found the most efficacious.

The absence from Dr. Lyons' Report of recorded loss of sight in even a single instance is most creditable to the institution with which he is connected. In the Mater Misericordiæ Hospital vision was lost by sloughing of the cornea in two instances; but amongst the most remarkable characteristics of the present epidemic, and by which it is distinguished from previous visitations of small-pox, is the comparatively small percentage of cases in which the eyes are at all affected. In certainly not more than half a dozen instances out of a total of 429 cases in the Mater Misericordiæ Hospital has the conjunctiva been pustulated. In connexion with this subject I may also notice the absence of pericarditis as a complication of small-pox. In only two instances, and in these the disease was very mild, in the "dry" form, and announced by physical signs exclusively, was pericarditis observed in the course of small-pox in the Mater Misericordiæ Hospital. There is likewise a much greater exemption from pitting than in former epidemics, and with this satisfactory result, the type of the disease, rather than special treatment, is, in

my judgment, to be credited. I have myself used, for the purpose of preventing pitting, equal parts of the *liquor plumbi diacetat. dil.* of the Pharmacopœia, and olive oil, smeared over the face twice or oftener daily, and have found it useful also in allaying irritation of the surface; this forms a liquid soap, and laid on thickly protects the skin from the contact of air.

Whilst on the subject of treatment I may be permitted to observe that I cannot subscribe the opinion expressed by Dr. Henry Kennedy at a late meeting of the Society, to the effect that the use of aperients in the stage of decline of small-pox should be deprecated, inasmuch as the resorption into the blood of the contents of the pustules is thereby promoted. I think, on the contrary, that this constitutes a reason for the administration of aperients which, moreover, promote the expulsion from the body of the undesiccated portion of the pustular contents, in which are probably comprised the bulk of the morbid matter, by one of the principal channels through which alone it can be eliminated, namely, the excrement organs.

In regard to the question of a "convalescent home" for small-pox patients, I hold with Dr. Lyons that such were better established in close proximity to, and in direct connexion with the hospitals which receive small-pox. The expense of a separate administration and of a distinct medical staff would be thereby avoided, and I doubt not the patients would have much less objection to enter such institutions than a distinct and distant establishment. I think Dr. Lyons has over-estimated the average period of convalescence in setting it down at six weeks. I should say the period of convalescence, as distinct from the actual duration of the disease, would not, on the average, much if at all exceed half the time assigned by Dr. Lyons.

In regard to the limited period of protection conferred by inoculation with variolous matter, or by a previous attack of small-pox, the following facts afford conclusive affirmative evidence. 1. A man inoculated in childhood had small-pox at the age of twenty, and was pitted by it; he again took small-pox at the age of forty, and made a good recovery. 2. A man had small-pox in childhood, and was pitted; he took it again at the age of thirty-eight, and recovered. 3. A female child who had small-pox in infancy, took it again at the age of nine years, and died. 4. The sister of the last-mentioned patient had small-pox when an infant, took it again when eleven years old, and also died. 5. A boy had small-pox when eighteen months old, took it again, in the confluent form, at the age of fourteen years, is still under treatment and progressing favourably.

So far, then, as the evidence at my disposal warrants me in forming an opinion, I should say that successful vaccination confers immunity

from small-pox for a period of about *nine years* ; and that inoculation with variolous pus efficaciously performed, or a previous attack of small-pox, may be held to protect from a subsequent attack for about the same period. Further, the conclusion has been forced upon me by what as a close observer of this epidemic I have witnessed, namely, that re-vaccination is not only necessary in the case of those exposed after the above-mentioned periods, but that it is an efficacious prophylactic against at least a primary attack of small-pox. The only examples to the contrary within my knowledge are those previously mentioned, and as to the sufficiency of the cicatrix I have personal and satisfactory evidence in only one out of these four cases.

DR. HUGHES.—I can endorse all the statements made by my colleague, Dr. Hayden, with regard to the statistics of our hospital. Dr. Lyons says, “confluence is not so essentially a fatal element,” and again he says “this was a highly purpuric form of the disease in which no other than a fatal result could be anticipated.” I do not know to which of these two opinions he adheres ; for my own part, I incline to favour the latter one. Of the 400 cases mentioned by Dr. Hayden I have had to my own share 120 ; and of these I have taken notes, which I may yet bring forward. Of these 56 were confluent, and 59 discrete. Of the 56 confluent cases 16 were confluent on the face, the extremities, and the body, and of those 16 generally confluent cases 12 died. Of those 56 cases 26 were confluent on the face and on the extremities, and only 6 of them died. Of those 56 cases 14 were confluent on the face alone, and 2 semi-confluent ; none of them died. Therefore it would seem that the high state of confluence determined the result. Out of the 59 discrete cases only 1 died, and that was a case which might not be fairly set down to small-pox at all, for it was a highly purpuric case occurring in an intemperate young man of 30 years, and I think that no matter what disease occurred to him he would have died. Of the vaccinated cases I find that out of 115 51 were imperfectly vaccinated, and 46 well vaccinated. Of the former 29 were confluent and 22 discrete. Of the 46 well-vaccinated cases only 10 were confluent, and 36 were discrete. Of the 16 unvaccinated and the 2 doubtful, they were all confluent. Out of 115 cases I had 19 deaths, which is equal to 15 per cent. nearly. Of those 19, 10 were vaccinated, and 9 not vaccinated ; and in all those 19 cases, with the exception which I have already mentioned, the disease was confluent. I wish to know whether Dr. Lyons has observed, if where the cases were well-vaccinated the protection to the patient was directly in proportion to the number of pits the scar contained.

DR. GRIMSHAW was sorry Dr. Lyons had brought forward a paper

such as this, at the present time, when the epidemic was still at its height. It is a well-known fact that the rate of mortality, and the various symptoms of epidemic disease, vary during the course of an epidemic, and that no definite statistical results can be arrived at until the epidemic has subsided. He (Dr. Grimshaw) also regretted that any remarks should go abroad with the authority of so eminent a physician as Dr. Lyons, which might tend to create a public opinion either that small-pox was non-contagious, or that contagion was of little account in the spread of such an epidemic as the present one. Dr. Grimshaw considered that contagion was the chief means by which the present epidemic is and has been promoted. Dr. Grimshaw had, at a previous meeting, informed the Society of how the majority of the cases admitted into Cork-street Hospital at the commencement of the epidemic were traceable to England. The statistics of Cork-street Hospital gave results almost identical with those just given by Dr. Hayden from the Mater Misericordiæ. In conclusion, Dr. Grimshaw suggested the appointment of a committee to collect all the available statistics with regard to the small-pox epidemic in Dublin.

DR. CAMERON.—I think Dr. Lyons has been only a little ambiguous, and that really after all he has not expressed himself a believer in the non-contagious theory; for when we find him so frequently referring to contagion, and stating towards the latter part of his paper that it would require six weeks before a patient ceased to be dangerous, it is quite evident that he believes in the communicability of the disease. He appears, however, to think that there are two modes by which the disease may be communicated; one by means of matter that is thrown out from the bodies of the affected, and the other, some mysterious way by which it is evolved out of the air. He states that the Swedish sailor having been fourteen days at sea, contracted the disease, and that there was not small-pox at the port from which he shipped. In my inspection of ships, when this port was under quarantine with respect to cholera, I found that vessels coming from the Baltic ports generally touched at Scottish and other ports, and in some of which there was small-pox. It is a remarkable fact that a month after the death of this sailor a gentleman who was attending at the hospital caught the disease. I think that expression of Sydenham—"epidemic constitution of the atmosphere"—a very unfortunate one. Dr. Brady could not have got the disease out of the air of the building, but out of the clothes of some person, or from the walls, or some other solid part of the building. Therefore, we find that the poison had lingered a month in the hospital, and then became active in the body of Mr. Brady. I think Dr. Lyons carries the idea of receptivity too far. I do not think the human constitution is always undergoing great changes. I believe,

speaking of great masses of humanity, that it takes a long time to undergo such changes of constitution as those to which Dr. Lyons refers. Extreme filth and total disregard of the conditions upon which the maintenance of health depends are factors that must be taken into account, and not any change in the nature of our bodies, which render them at all times apparently more disposed than another to disease. I do not hold that our population at large undergo the periodic changes that Dr. Lyons implies. Putting the clinical pupils and others with the visitors, we have 157 persons, and of these 14 were affected by the disease, equal to $9\frac{1}{2}$ per cent., a very large proportion to the number of persons subjected to the contagious influence. Out of seven resident clinical clerks two took the disease, a most enormous proportion. Dr. Russell, on the Glasgow Fever Hospital, shows by statistics extending over a long time, that every one, from the medical men down to the man who distributes the bread, and the man at the gate, gets fever. We have Cheyne showing that this disease is communicated from individual to individual, and that it does not arise out of any state of the atmosphere. I find that in the London small-pox sometimes 87 per cent. of the patients have been vaccinated. It appears to me that vaccination is not so useful in preventing persons catching the disease as it is in modifying the disease when contracted. Unless we admit that vaccination has been very imperfectly carried out in this country, we cannot account for the large proportion of cases of post-vaccinal small-pox occurring in Dublin. Before stating that this is a young epidemic it would be well to know what is the average age of the population of this country. I believe there are more persons in the country under than over twenty years. I think the Society should feel indebted to Dr. Lyons for his valuable paper, and I cannot agree with my friend Dr. Grimshaw in considering it as premature. It is well during the height of an epidemic to discuss questions relating to it, as it might probably suggest improved modes of studying the phenomena of the disease.

DR. DARBY.—The first case that I know of as having occurred in Bray is that of a young woman who attended in a shop and resided in another house. There was no other case occurred in either of the two houses she was in. Contagion appears to me the most important point of all. I believe the disease is communicated from the sick to the healthy, but I do not believe that it is limited to a certain number of inches from the bed; nor that it can be conveyed from one distant country to another. One of the first men in the profession in Dublin has stated that it was communicated by a letter. I am a believer in the epidemic constitution of the year. I do not see any other way why we should have small-pox this year and two or three years ago none; why we should have cholera raging through the country, then disappearing;

and so of scarlatina, and all zymotic diseases. All I contend for is that you must not have a germ, that you might have the disease communicated from the body of the sick man to the healthy. I believe you can communicate small-pox or any other disease. I have seen cases where I was perfectly satisfied that disease was carried from the sick to the healthy. But that does not account for why we should have the cholera here—going away and coming again. I say there is no such thing as blood-poisoning. When there is no epidemic you will have cases of scarlatina in a school and it will not spread. It will spread like wild-fire on one occasion, and not spread at all on the other.

DR. CHARLES MOORE.—In 1869 there was a case of a Swedish sailor to which Dr. Lyons has referred. In the Poor Law report it is shown that that case gave rise to another near Maynooth, and that then other cases arose. A young man lying in the Hardwicke Hospital, near the Swedish sailor, went to Maynooth, and got attacked with small-pox. There was also the famous clothes case from Liverpool, which gave rise to several other cases.

DR. AQUILLA SMITH.—If we were to get statistics of any practical interest they would pay more attention to the character of the eruption, for, as statistics are drawn up, it is statistics of a name—of an affection called small-pox—that we have. Fourteen days is about the time laid down by statistical writers as the term of incubation in small-pox, and that strikes me as an interesting point in the case of the Swedish sailor, who was at sea 14 days before he was observed to be ill. There is very little difference in the mortality laid down by Sydenham and that observed at present. Free pustulation has been alluded to, and Dr. Lyons has been kind enough to introduce my name. I consider, from a very close observation of this epidemic, that the character of the eruption, and the order in which the eruption presents itself, is one of the highest and deepest importance in watching and studying the disease. I saw a considerable number of cases where the eruption was very copious, and to some extent confluent, and those cases invariably did well. A very small proportion of the cases exhibited marked swelling of the face; but all those that did did very favourable, no matter how close and copious the eruption. I have seen a large number of cases with total confluence, and they did well. In those cases where there was a fair vaccination mark, I observed in a great many that the disease ran to a state of maturation very speedily. There were many cases in which the disease did not proceed to suppuration at all—the eruption being abortive—in many cases not going beyond the papular stage—in other cases the fluid being only a whey-like fluid. I cannot admit that treatment prevents loss of

sight. Loss of sight depends on the formation of an ulcer on the cornea, and its eating its way through. I believe that blinding by small-pox is in consequence of an ulcer forming on the cornea, and that there is no means of preventing it. I have used laxatives very frequently—even where the eruption presented freely—and diarrhœa set in; possibly from great quantity of matter absorbed into the system. I did not attempt to check the purging.

DR. WILSON.—The majority of eyes lost in small-pox are lost through ulceration of the cornea. Sight is also lost by iritis. In this epidemic I have seen two or three cases of iritis—one most obstinate—in which we had great difficulty in saving the sight. I must differ with Dr. Smith, as I do not think small-pox pustules occur on the cornea. It is not a small-pox pustule that occurs on the cornea. I wish to draw attention to the infrequency of eye disease during the epidemic. I have seen only six cases of eye disease in small-pox since the beginning of 1871.

DR. LYONS.—If I have done no other good, Sir, I have elicited a vast amount of most valuable information which we probably should not otherwise have had for a considerable time to come, in regard to the present epidemic. In reply to the observations that have been made to-night, I would, in the first instance, call attention to the fact that all the gentlemen who have spoken have, in a singular manner, omitted to take any notice of the table I have prefixed to my statistics, which gives an account of the small-pox cases that have passed through the Hardwicke Hospital for the last thirty years. That table, Sir, is a conclusive proof that although small-pox had decreased to a very large extent in this country, it never had wholly and finally ceased. A large number of cases, amounting to 1,296, are reported in that period of 30 years. Furthermore, when gentlemen speak of the first case on record of this epidemic, they entirely overlook the fact that cases continue to occur during the year 1870, in the City of Dublin. I have reported here, from the records of the Hardwicke Hospital, certain cases, and it is consistent with my knowledge, and probably also with that of many gentlemen present, that other small-pox cases occurred throughout the City within that year. I have mentioned the case of William Knowles, who was admitted on the 4th of April, 1870, from the Richmond Lunatic Asylum, and discharged cured; and that of Richard Hynes, who was admitted on the 27th July, 1870, from Maher's Court, North Brunswick-street, and discharged, cured. I put all the cases together; and I put forward these two cases in particular, to show that though small-pox had subsided as an epidemic, it was still an endemic disease amongst us, and that examples of it were to be found here and there throughout the community. I showed that in the course

of the year 1871, at least two cases presented themselves under circumstances in which the individuals had not been out of the country at all, and in which importation was wholly out of the question. With regard to importation into this country, it is like bringing coals to Newcastle, to pursue such an inquiry, for small-pox existed as a latent endemic disease, not spreading widely amongst the public, showing no tendency to an epidemic character; and it would be perfectly impossible to say that it had been absolutely eliminated from the country. I have stated, on the authority of the Registrar-General's Report, that no death from small-pox was reported in the last quarter of 1869, and Dr. Burke corrected me by saying that the return was that for the Dublin district, and not the whole of Ireland. However, I find that statement in print in the report in reference to the whole of Ireland. With regard to Sweyn Johanson, the Swede, who came into hospital in November, 1869, I may remark that the captain of his ship was a man of unusually intelligent mind; that he spoke English exceedingly well, and that he gave a succinct account of the history of the ship, and stated that the ship did not touch at any intermediate port. There is, therefore, no ground for supposing that the disease was taken in at any port at which the vessel touched. The case was an extraordinary instance of the most virulent form of small-pox that could be met with, and beyond the one individual, Dr. Brady, who has been supposed to have taken the disease from Johanson, I know of no others who did. I am sorry Dr. Charles Moore has left the room, for he made allusion to some alleged facts with regard to the alleged transmission of the disease from Johanson to individuals in the neighbourhood of Maynooth. A statement of that sort was circulated, and made the subject of a Poor Law inquiry; but it so happened that two distinct cases became mixed up together, and when I say that the report of the case was made by the late Dr. Hill, you will understand how deeply I feel the gravity of what I say, when I state that he was unfortunately led into error as to the facts with regard to the supposed spreading of the disease. Dr. Hill made inquiries at the hospital when I was not there. I was in charge of the girl Anne Farrell, who was said to have communicated the disease to Maynooth. I have to ask the indulgence of this Society for not going into the proofs with regard to it, as they would take us a long way from our present inquiry, and I have to ask you to believe that those facts were not as has been stated, and that no proof was urged as to the disease having spread from Anne Farrell or Sweyn Johanson. I most fully inquired into all the circumstances. Dr. Hayden has favoured us with some extremely acute and very able criticisms on my paper. He calls attention to the percentage mortality in regard to the last decade. The numbers, however, are exceedingly small in that decade. The disease did not show

any tendency to spread; it was very light in character, and the death-rate was exceedingly small. Therefore we are not comparing like conditions of disease, or like states of epidemic visitation, when we compare the deaths in an epidemic visitation of great severity with the deaths occurring over a long series of years in the period from 1861 to 1871. Cases having existed in this city prior to the case to which Dr. Hayden has alluded—that of Eliza Delany, admitted on the 3rd February, 1871—I say, Sir, that it is a wholly gratuitous assumption, and begging the whole question of “importation,” “epidemic constitution,” “contagion,” or whatever else you like to call it, to say that that girl got the disease from washing the clothes of the soldiers. It is a very easy theory to account for it. It is a very inviting peg to hang a theory on. But all dispassionate men must agree with me that it is a case absolutely non-proven; that it is an assumption, and nothing more. I cannot, therefore, agree with Dr. Hayden in the conclusions he draws, and his arguments would not, I am of opinion, bring conviction to any logical mind. With regard to the mortality in the Mater Misericordiæ Hospital and the Hardwicke Hospital, I trust that no discussions in this Association will ever be allowed to have a tendency to contrast the doings in one institution with those in another (hear, hear). We come here, Sir, with one purpose alone—to bring the results of our observation in their full naked truth and simplicity before a scientific meeting (hear), and not to draw invidious comparisons as to success in one institution or another. It so happens that if my communication had been made on the first occasion that I proposed, the percentage mortality on the even number of 500 cases would have been very much below that on the 612. A flood of bad cases—purpuric cases—came in amongst the additional number, and that bore down our mortality, so that I had to present results that are not as favourable as they would have been if deduced from the smaller number of cases. Dr. Hayden himself must be aware of the fact that taking cases by the hundred, or fifty, the mortality will vary in a most capricious manner. With regard to the first hundred cases that passed through my own hands, I thought I was the greatest small-pox doctor in the world, the mortality was so small. If we dissect the mortality in the cases I have brought before you, we shall obtain some results contrasting favourably for me. Of the cases that passed through my own hands, the mortality is considerably less than the total mortality; but I do not think it would be fair for me to separate the statistics of mortality of the cases that passed through my own hands from those that passed through the hands of my very distinguished colleagues. But as the argument has arisen I will mention a few points. The mortality is not only different in regard to different groups of cases in the epidemic, but it is different in different wards of the same hospital.

On the whole of the cases admitted and treated immediately by me, which happened in consequence of my having charge of two supplementary wards, to be larger than my proportion, the percentage mortality was smaller than that on the total mortality, the mortality on my own cases in gross being 17·50 per cent. We established two supplementary wards, which were plainly, simply, and it might be said even rudely got up. They were the least presentable of the wards, and they were the wards to which I would perhaps last take a visitor. Yet these were the wards that presented the most remarkable proportion of recoveries. One of these wards—the male supplementary—shows a mortality of only about 12 per cent., and the other ward—the female supplementary—which was a flagged room, shows the absolutely unparalleled mortality on a considerable number of cases of only 4·87 per cent. Therefore, we cannot argue from any single group of cases, or from any small number, and if any argument were to be drawn it should be from a precisely similar number of cases. I have already shown how singularly the percentage of mortality varied from month to month. We should take a certain number of cases from one institution and compare them with a similar number in another for precisely the same periods of the epidemic. I hope that Dr. Hayden under like conditions will mention his results with regard to re-vaccinated cases, which I regret to learn did not show a favourable result. I have mentioned three cases, amongst which there were no deaths. If Dr. Hayden has any information on that subject, I would ask him to favour us with it. As to transmission of the disease from one set of individuals to another, and to the attendants, the records that I have put before you are very striking. They are, in some respects, contradictory. With regard to re-vaccination, or any additional precaution for the nurses and attendants, all facilities were afforded them for re-vaccination; we recommended it, but we had no means of enforcing it. Only two subjected themselves to it; in one it failed, and she declined to submit to it again. I cannot say, therefore, that any considerable number of those nurses submitted to re-vaccination, or that any unusual care was taken on their part to prevent contagion. In fact, as happens in all cases during the prevalence and height of an epidemic, persons became callously indifferent to the danger, and it is astonishing to see the close watching and personal attention they give to the worst cases, and how little they fear the effect of contagion; and certainly, if confidence gives safety, it has been so with the vast majority of the nurses that had charge of our wards in the Hardwicke Hospital. I have stated that but one female attendant—a ward-maid—took the disease. Dr. Hayden has made some very interesting and highly valuable observations on a singular form of the disease that has presented itself in the present epidemic, all of which I can endorse.

Besides the purpuric cases, rubeoloid cases, and cases of a scarlatinoid character presented themselves during the epidemic. In some instances it was impossible for days to say what was the exact nature of the malady, and, curious to say, after the subsidence of the small-pox in certain instances, fresh eruptions of one kind or another presented themselves. One singular instance was that of a man who rapidly convalesced from small-pox, and after he recovered a singularly vivid scarlatinoid rash appeared, of which I would have said, if he had not the distinguishing characteristics originally, "he is about to be one of our purpuric cases." I thank Dr. Hayden for calling my attention to the question of pericarditis as a complication of variola. In dealing with such large facts as I have had before me, perhaps the Association will excuse me for having omitted and forgotten many important details. I was anxiously on the look out for pericarditis and other cardiac complications. Two cases occurred; the double friction sound was well marked in both instances; neither of them died. In connexion with some very remarkable forms of the purpuric variety of the disease a well-marked systolic hæmic murmur was observable during the worst period in the purpuric condition. I may also note that in at least three instances renal disease of some form followed. We had no opportunity of *post-mortem* examination where dropsy associated with albumen in the urine occurred in three males subsequently to the disappearance of the varioloid eruption.

With regard to the period required for convalescence—three or six weeks—that is a matter of experience and not of discussion. My own opinion, based on experience is, that it would not be safe to say that the patients would be free from conveying contagion in a less period than six weeks in a large number of cases. Dr. Hughes has called attention to my statement in regard to the question of confluence, and he refers to two statements which he thinks contradictory; but I do not: I adhere to both of them. I have stated that confluence is not so necessary a determining element of death as has been commonly supposed, for I find that out of 203 undoubtedly confluent cases only 104 died, that is a little more than half died, and a little less than half recovered. We were very particular in designating confluent cases, and I will in brief summarize what I have to say on that subject, that no case was called confluent that had not a considerable amount of confluence on the face, trunk, and extremities. With regard to the question of vaccinated cases, and cases well or badly vaccinated, I entirely agree with Dr. Grimshaw. I have examined the marks with great minuteness, and I will freely confess my ignorance by saying that I am incapable of determining what is such a perfect vaccination mark as will protect the patient from getting small-pox, or if he gets it will determine whether or not he will have a bad form or whether he shall die or not. I have seen the worst cases die though well-marked

cicatrices existed, and I have seen light cases recover with very superficial and imperfect marks.

Dr. Hughes has made a remark as to the patients in the Mater Misericordiae Hospital going about the wards without going to bed. Those are like the cases we heard of from Dr. Burke and Dr. Maunsell, of persons "putting the disease over them standing!" Unfortunately we had no such cases, and I must say that the great majority of the cases admitted into the House of Industry Hospitals were of an excessively bad type.

With regard to the question of contagion, I beg leave to say that I never said that I did not believe in contagion. I do believe that the disease is contagious, and directly communicable from one individual to another. But what I say is that contagion is not the sole agency concerned in propagating the disease; that given as bad a case as you like to start with the disease will not spread as an epidemic without some other unknown agencies, or conditions other than contagion—conditions that I shall not pretend to define; but beyond any doubt contagion is not the sole agency, the sole factor in producing the spread of this frightful disease or of any other epidemic. If contagion were the sole or even the main agency, where would the epidemic ever stop—where would it begin—where would the first and last case be? If contagion is to be propagated through the Grafton-street perambulators referred to by Dr. Grimshaw, the wonder is that the whole city do not get it, and that it does not spread to 200,000 individuals or to the whole population. I would also point out to the contagionists this consideration, how does the disease ever cease if contagion is the main agency in developing it. It cannot cease if contagion is the sole agency. You cannot trace the beginning or end of contagion if it is contagion. I have some important facts and arguments to adduce, but will not now discuss them, as to what are the conditions or agencies requisite for spreading any epidemic disease.

With regard to Dr. Cameron's remarks as to the epidemic influence in the propagation of disease, we cannot read the history of the epidemics that have decimated the world from time to time, without recognizing some influence of a mysterious and unknown quality that presides over the origin and spread of disease. How, otherwise, is it that the plague has ceased in the Levant, that the sweating fever no longer exists in this country, and has not for two centuries though I have seen it in the East, and how could it be confined to one race—the Turks, and not spread to the English, the Sardinians, or the French? I think Dr. Cameron has not given proper weight to epidemic influence. I would wish to correct Dr. Cameron's strictures on the use of the term "epidemic constitution." Sydenham used the term as I understood him, not so much in reference to the material conditions which are present in disease, as to the character

of the epidemic itself at the time. That is the point on which he lays so much stress in reference to prognosis—that you should never, until you have recognized the epidemic constitution, hazard a conclusion. It is the terrene or material conditions that preside over the growth and propagation of the disease, but the character of the disease itself that he alludes to—in fact the lethal co-efficient of the disease, whether it be one of severity, killing at a high rate per cent., or of a lesser degree of severity. We know that epidemics differ much in their lethal co-efficients in a very singular manner. With regard to Dr. Russell's statistics alluded to this evening, I believe there is a considerable element of fallacy in them. He states that all the individuals connected with the Glasgow Fever Hospital caught the disease sooner or later. I would ask is the population of an hospital a constant population? Our experience is that it is a constantly changing population; you lose sight of a large portion of them; your clinical clerks, nurses, and wardmaids change year by year, month by month. I am glad to be able to give Dr. Cameron some additional information with regard to the spread of the epidemic amongst the students. Dr. Cameron knows how difficult it is to get the facts from a large number of scattered individuals. We find that amongst the students, of whom I have now an accurate record, five non-vaccinated students took the disease; seven students had small-pox altogether, so that strengthens Dr. Cameron's argument as far as it goes. But here is a very important statement that I think it worth while to make known—twenty students were carefully re-vaccinated and took the vaccine poison, and though they were all constantly exposed to the disease no one of them took it.

DR. GRIMSHAW.—All the students in Steevens's Hospital were re-vaccinated, except five, and those five got small-pox.

DR. LYONS.—That is very important. Twenty gentlemen in our hospital were re-vaccinated, and of these, none took small-pox. Three were found not susceptible of vaccination, and they did not take small-pox. Dr. Darby made an important statement with regard to the case of a girl at Bray, but he was not able to put us in possession of the date, and there are many points about the case that I would ask him to clear up. With regard to the post-bag precaution, it reminds me of what I have seen carried out in quarantine stations abroad. No letter would be received from a suspected port without being submitted to a fumigatory process, and the incoming postman deposited the letters in a chamber isolated by iron bars. The letters were thrown into an enclosed space and fumigated, and then picked out by means of an iron tongs of great length. Unless a process like this is carried out carefully the most absurd things might happen to the public. Thus Dr. Darby's friend might be afraid to

take a letter out of the post-bag, as another letter, which might come from a small-pox locality, might be in it. This really leads us to absurdity. I mention those points because they have an important public bearing. I hope Dr. Cameron will not push his dicta so far as that we shall be driven to the necessity of adopting in this city those quarantine regulations which have proved so utterly inefficient, and which are in practice so futile and ridiculously absurd, as well as being quite inoperative, which prevail in those still dark regions where theories of importation of disease and contagion rule the roast. Anything more ruinous to the intercourse of one community with another cannot be conceived than a system of quarantine, and anything short of a perfectly strict system would be utterly futile. If you have the cattle dealers coming to the North Wall subjected to quarantine, you must have the lords and ladies, the Lord Lieutenant and Chief Secretary, and their baggage, subjected to quarantine when they disembark at Kingstown. Of course that is a project which could not be carried out in practice. I believe that infinite mischief would result from such a course, and I trust that Dr. Cameron, in his important relations with the public and the authorities, will never allow his theories to be pushed so far as to induce gentlemen to get up a system of partial quarantine in this city. Anything more vexatious and useless, and I would go the length of saying ridiculous, cannot be found than the quarantine regulations as they exist in Spain, Portugal, and along the shores of the Mediterranean; and, as has been admitted to me, they are more used for political objects than for sanitary purposes. Dr. Moore has given us a very interesting and most important case. I do not deny that a case might come from Glasgow or elsewhere. But the most curious part is, that after that case communicates itself to a certain number the disease seems to become worn out: the contagion goes to a certain extent, and there ceases. Dr. Aquilla Smith has exhibited a very interesting subject, a drawing of a doctor in his coat of armour to prevent him from taking epidemic disease; but he omitted to mention that the beak was evidently for the purpose of picking up the infected fees and placing them out of reach of contamination. I am glad that Dr. Wilson has favoured us with his observations on affections of the eye. Corneitis and iritis have been in one or two instances exhibited, but we have overcome those affections, and out of a still larger number of cases than I have brought forward not one has as yet lost an eye.

With regard to purpuric cases, we have had some very remarkable instances of that form of the disease since I last addressed you. Some very striking instances occurred, in which not a single pustule was formed on the surface of the body. I am happy, to correct to some slight extent, the observations I made as to the fatality of this form of the disease in my first communication. I have since seen some remarkable cases recover.

One very remarkable case, with hæmorrhage from various organs and blood in the urine, black circles around the nipples for an inch and a half in extent. That case recovered under perchloride of iron and lime juice, which I would recommend from the success of this case.

I think this exhausts what I have to reply to. I have only to thank the Association and all the gentlemen who have spoken, for the great consideration and kindness with which they have received my observations. I cannot agree with those who think statistics of no value, nor do I think it premature to heave the log and see the rate at which we are going, and what is the mortality of the disease. It will be in the recollection of the gentlemen who heard me, that I proposed in the end of my paper that a committee should be appointed of members of this Association for the purpose of making further inquiry into this subject. I will most cordially join Dr. Cameron and the other hospital physicians present in getting up the committee. I think it would be most desirable that the dispensary physicians should be associated with the committee. I think a committee of six or eight gentlemen having hospital and dispensary information could give us by the end of next year a complete *resumé* of the history, pathology, and clinical aspects of this most remarkable epidemic.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

Saturday, 20th April, 1872.

LOMBE ATTHILL, M.D., Honorary Secretary.

The President, DR. KIDD, in the Chair.

Report of Private Obstetrical Practice for Thirty-nine Years. By FLEETWOOD CHURCHILL, M.D., Dublin and Edinburgh; formerly President of the Obstetrical Society, &c., &c.

THE state of my health having rendered night-work impracticable, I consider that my special obstetric practice closed with the year 1870, embracing thirty-nine years complete. I propose now to lay before you "an account of my stewardship," with a hope that it may not be without some value as a contribution towards the general statistics of private practice, and perhaps as a help to my younger brethren. It seems the more fitting, because I began my literary life with this Society, and by far the larger number of the papers I have written have been submitted to it. I do not suppose that the results of my practice are more favourable than those of other practitioners, and their only merit is that they were entered in my case-book at the time they occurred. At the commencement I drew up a tabular form (of which I show you a page), so that a case could be entered in a few minutes, with a blank book for cases which required full details. In making the following summary, I have found the information more imperfect or defective on some points than I should have wished, but as the cases were recorded at the time and nothing trusted to memory, I believe I may claim perfect accuracy for what is there recorded.

The period embraced in this report is from January, 1832, to December, 1870, inclusive, during which time I attended 2,547 cases of labour. I do not include in this number premature labours before the seventh month, nor abortions; of which latter I have 140 cases recorded, but many were omitted.

In the accompanying table will be found the number of labours in each month of each year. In the order of frequency the months were as follows:—April, May, March, August, July, October, January, December, June, September, November, February.

In 2,540 cases of which there is a note, I find there 1,290 males and 1,250 females; of the entire number there were 130 deaths within the

week, or 1 in $19\frac{1}{2}$. There were 16 cases of twins, with 11 deaths, and one case of triplets. The deaths include cases of premature labour, putrid children, and operations.

	Jan.	Feb.	Mar.	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Total
1832	1	—	—	—	—	—	—	3	—	1	—	—	5
1833	1	1	—	2	—	—	1	1	—	—	1	—	7
1834	1	1	1	—	4	—	1	2	—	—	2	—	12
1835	2	4	—	—	2	—	—	1	2	1	1	2	15
1836	2	2	1	1	2	1	—	3	3	—	1	1	17
1837	2	3	—	1	4	1	9	7	3	3	2	6	41
1838	4	4	3	6	4	5	1	3	1	6	3	—	40
1839	—	—	3	3	5	5	5	7	3	5	10	3	49
1840	7	4	6	4	6	6	8	3	2	6	3	4	59
1841	5	6	8	6	1	4	4	7	5	4	2	2	54
1842	2	5	6	10	6	1	1	8	7	6	4	5	61
1843	5	3	6	8	8	7	4	4	7	3	4	6	65
1844	6	5	6	6	6	6	6	6	4	2	3	10	66
1845	4	2	7	13	9	4	4	5	9	3	6	8	74
1846	5	5	5	11	4	3	5	9	7	5	9	4	72
1847	6	5	6	4	6	5	3	9	10	7	8	5	74
1848	9	6	7	8	11	2	7	6	8	10	9	7	90
1849	3	8	10	4	4	2	7	2	9	4	7	8	68
1850	5	5	5	6	5	6	7	13	9	7	4	8	80
1851	4	2	5	7	4	5	9	5	8	5	9	8	71
1852	6	3	7	6	12	4	11	5	4	8	3	5	74
1853	10	6	12	5	8	6	6	6	4	9	4	5	81
1854	9	11	4	7	6	6	8	2	9	8	3	11	84
1855	7	4	7	8	5	8	5	11	8	6	7	6	82
1856	7	5	7	9	6	10	4	10	7	9	2	7	83
1857	8	6	5	7	3	8	5	3	9	7	9	7	77
1858	10	4	6	7	8	8	20	8	4	7	8	9	99
1859	7	4	9	7	1	5	7	9	8	7	13	6	83
1860	7	5	10	14	17	6	4	9	7	6	11	12	108
1861	12	9	5	10	8	10	12	6	5	9	5	8	99
1862	5	7	12	10	14	11	6	9	3	6	5	5	93
1863	10	8	11	8	9	13	12	7	6	5	5	3	97
1864	6	6	10	8	8	8	3	6	2	6	7	7	77
1865	3	4	9	11	12	8	8	7	4	10	8	5	91
1866	5	9	7	8	5	6	7	9	7	13	3	7	86
1867	10	8	6	1	3	3	5	4	6	5	4	8	63
1868	7	7	6	7	8	7	5	8	2	6	4	1	68
1869	4	4	7	5	5	5	9	2	3	2	4	2	52
1870	1	2	2	5	5	3	0	2	1	3	3	5	32
	208	183	227	243	234	198	219	227	196	210	196	206	2,547

Now as to the duration of the labours, I find in the 2,547 cases there were 47, or 1 in $54\frac{1}{2}$, in which the entire labour lasted beyond 24 hours, but no death occurred among these cases. Between 12 and 24 hours there were 161 cases, or 1 in 16, with two deaths—one from phthisis, who just survived delivery a few days.

In trying to ascertain the length of the second stage, one is met by the

difficulty of fixing its beginning accurately. I took as a general rule the rupture of the membranes, excluding, of course, all cases where I knew that this occurred at the commencement of labour. With this limitation I find it recorded that in 54 cases the second stage was 4 hours; in 36, 5 hours; in 12, 6 hours; in 20, 7 hours; in 5, 8 hours; in 4, 9 hours; in 4, 10 hours; and above 10 hours in 7 cases. I do not find any death recorded among the prolonged second stages, but I may honestly confess that in some of them, with my present experience, I should rather apply the forceps than allow them to run on so long.

Of the interval which elapsed from the birth of the child until the extrusion of the placenta—*i.e.*, the third stage of labour—I have a record in 2,387 cases. I find that in 1,965 cases it was 5 minutes; in 278 it was 10 minutes; in 61 it was 15; in 25 it was 20; in 27 it was 30 minutes; and in 8 cases it was an hour. There is a note attached to most of these latter cases, stating that I did not arrive till some time after the birth of the child.

I have found only 3 cases of *post-partum* hæmorrhage, with one death.

There were 10 additional cases in which extraction was necessary for different reasons, such as flooding, irregular contraction, and morbid adhesion. I may, perhaps, be allowed to remark that many of the cases in which the longer intervals elapsed occurred in the earlier years of my practice, before I had realized the safety and value of pressure so applied as to squeeze out the after-birth from the uterus into the vagina.

If firm grasping pressure be applied to the uterus immediately after the child is born and continued for a few minutes steadily, it will be found to diminish in size suddenly, and the placenta will be found in the vagina, from which it is easily removed. The squeezing force should never be excessive, lest we inflict injury, nor is it necessary. More than twenty years' experience has satisfied me not only of its efficiency but of its safety. I have never known hæmorrhage follow in cases thus treated. In some few cases it will fail of course, and then we must have patience until uterine action sets in, or other circumstances show the necessity of further interference.

In 2,298 cases I have entered the measurement of that portion of the funis attached to the placenta. In 7 cases it was 14 inches; in 17, 16 inches; in 7, 17 inches; in 291, 18 in.; in 129, 19 in.; in 769, 20 in.; in 212, 21 in.; in 417, 22 in.; in 7, 23 in.; in 99, 24 in.; in 4, 26 in.; in 13, 28 in.; in 11, 30 in.; in 1, 32 in.; in 11, 36 in.; in 3, upwards of 40 in.—one of them being 56 inches in length, and coiled round the child's neck and body.

To this portion of the funis must be added the inch or two remaining attached to the child, and then we shall have, I think, a pretty correct notion of the comparative length.

I have never seen a cord so short as to occasion inconvenience or to require division before the child could be born. In most cases above a certain length, coiling round the neck is common, and I believe more frequent in proportion to the length of the cord, the obvious effect of which is to diminish the chance of prolapse. The longest cord I ever saw was not prolapsed, probably because the excess was employed in coiling round the neck, body, and arms of the child.

To complete what I have to say about the funis I will state the days of the decadence of that portion which remains attached to the child in 1,528 cases. It fell on the 3rd day in 5 cases; on the 4th in 52; on the 5th in 407; on the 6th in 710; on the 7th in 307; on the 8th in 29; on the 9th in 9; on the 10th in 6; on the 11th in 2 cases; and on the 12th day in 1 case.

I do not know that this trifling matter is of any practical importance, but one may as well know all one can accurately, and it is certainly an illustration of the value of tabular entries in accumulating facts with the least possible trouble.

I now come to speak of the *presentations* in 2,565 children.

There were 6 presentations of the superior extremities, or 1 in 427. 3 were dead-born, 1 putrid.

There were 49 breech presentations, including premature cases, or 1 in 52½. 16 were born dead, 7 of them putrid. I am happy to find that as I gained experience I lost fewer breech cases. Most of the deaths occurred in earlier days.

There were 18 presentations of the foot or knee, or 1 in 142½. 9 were dead-born, of whom 6 were putrid.

There were 4 face presentations, all saved.

There were 21 cases of the forehead towards the pubis, of whom 2 were dead.

There were 6 cases of simple prolapse of the funis. 4 were born dead. 1 was craniotomized after pulsation had ceased. In 2 other cases the funis prolapsed with a foot presentation; 1 child was putrid.

There were 7 cases in which the hand or arm came down with the head. All saved.

In one case the foot and hand presented; the child was premature and putrid.

There was 1 case of spontaneous evolution. The child was lost, of course.

It must not be forgotten that this list includes premature labour at and after the 7th month, and we all know that abnormal presentations are far more frequent then than at the full time.

Of complications (*complex labour*), I find 1 case of convulsions. Mother and child died.

5 cases of accidental hæmorrhage. 2 of the children and 1 of the mothers died.

2 cases of unavoidable hæmorrhage. 2 children lost, but neither of the mothers.

1 case of internal hæmorrhage before the birth of the child, which was lost. Mother saved.

1 case of *post-partum* hæmorrhage. Fatal to the mother.

1 case of rupture of the uterus. Fatal to the mother in 20 minutes, and to the child.

I had laceration of the perinæum in a few cases, but none through the sphincter. Of late years I always carry a suture needle and silver wire in my pocket case, and in the three last cases of laceration I inserted immediately two sutures, and the wound healed at once. All have had children since, and I found the perinæum perfect, nor was it torn again.

8 cases of prolapse of funis. 5 children dead; 1 putrid.

3 cases of puerperal mania. 2 mothers recovered; 1 died.

The next matter I have to bring before you is the number and results of the *operations* I found necessary.

I.—*Version* was performed 7 times, or 1 in $363\frac{1}{2}$, with perfect success as regards the mothers. All recovered well.

In 5 cases it was necessary, in consequence of presentation of the hand, arm, or shoulder. 2 children were saved. 3 were born dead; 1 of them putrid.

In 1 case of prolapse of the funis version was tried to save the child, but it failed.

In 1 there was narrowing of the brim of the pelvis. The child was born alive, but died afterwards.

During the same period of practice I had in consultation 16 cases of version. 1 mother died of puerperal fever afterwards; all the rest recovered.

12 children were lost. 7 were dead before the operation, and several were premature.

II.—There were 42 *forceps* cases, or 1 in $60\frac{1}{2}$, with 3 deaths, or 1 in 14 among the mothers; 1 from cardiac paralysis, as I believe, and 2 from hysteritis. 4 children were dead-born.

33 cases were examples of powerless labour.

1 „ of internal hæmorrhage. Child dead-born.

5 „ of forehead to the pubis.

1 „ prolapse of funis. Child dead.

1 „ of phthisis.

1 „ of convulsions. Child dead.

24 of the cases are marked as first labours, but there may have been more, as the column is not filled up in all of them.

During the same period I have had 80 forceps cases in consultation, of whom,

1 mother died of peritonitis afterwards, and

1 was *in articulo mortis* when delivered. 5 children were born dead. 4 died shortly afterwards.

III.—7 cases of *craniotomy* are recorded, or 1 in 363.

In 2 I had recourse to it because of accidental hæmorrhage. Both were premature labours, and the flooding great. 1 mother recovered well. To the other I was called accidentally, not having seen her before. I found her all but dying from hæmorrhage, and it was by the advice of the late Dr. C. Johnson that I delivered her in the only way possible, to give her a chance, but she never rallied. He had no doubt but that the hæmorrhage had killed the child.

In 1 case of convulsions it was performed in hope of saving the mother, but she died.

In 1 case of impaction, after Dr. Beatty and I had failed with the forceps, we had to craniotomize, and with the less scruple as the long second stage and the great pressure left little hopes of the child being alive. The mother recovered well.

In 1 case of pelvic deformity I tried version and failed, leaving me no choice but perforation. The mother was attacked afterwards with phlegmasia dolens, and died in the country.

In 1 case of powerless labour, where the funis had prolapsed and ceased to pulsate; the mother recovered.

In 1 case of powerless labour, in which the fœtal heart had ceased to be audible after having been heard; the mother recovered.

I confess, Sir, I was rather surprised when I counted up these numbers. I have, as you know, felt myself called upon more than once to justify the employment of craniotomy, even when the child is known to be alive, *provided that it cannot be delivered alive by any means we possess*, and provided also that by being delivered, the mother may be saved. Yet, I do also hold, and have always taught that it should be our aim to limit the frequency of this operation as much as possible, and notably by the earlier and more frequent use of the forceps. This report shows that whilst I have in my own practice used the forceps 42 times, I have only had recourse to craniotomy seven times, and of these there were only three cases in which there could be any doubt of the child being dead. In two out of the three, the life or death of the child could not enter into consideration, since we had failed to complete delivery in the only other way open to us. In noting the mortality after any midwifery operation, we must not forget that in many cases the death results not only, or not at all, from the operation, but from the causes which render it necessary.

I quite agree with an observation made by you, Sir, in your inaugural

address—viz., that no operation should be performed without a consultation, when it is possible to obtain it, unless under circumstances of urgency, or for special reasons. I have never done so, and I am sure that these exceptional cases do not exceed six in number.

During the same period I have had in consultation thirty-one additional cases of craniotomy, with no record of the death of any of the mothers. In thirteen there could be no reasonable doubt of the death of the child previous to the operation.

Now, Sir, I come to the *maternal deaths*, which, I am sorry to say, in the 2,547 cases amounted to 17 or 1 in 149 $\frac{3}{4}$. I have not included in this number a case who left town ill after an attempt at version and delivery by craniotomy, and had an attack of phlegmasia dolens, of which she died. It is right, however, to mention it.

Of the seventeen cases, only eight can, I think, be fairly said to have died of puerperal fever, or 1 in 318. The following is a very brief summary of all the cases:—

1. First child; easy labour; attacked on the 3rd day, died on the 8th; lived in town; child lived.
2. Lived in town; easy labour; first child; attacked on the 9th or 10th day; died on the 16th; child lived.
3. Lived in the suburbs; easy labour; first child; attacked on the 8th day and died on the 14th; child died of erysipelas.
4. Lived in the suburbs; easy labour; first child; attacked on the 3rd day, died on the 8th; child lived.
5. Lived in town; easy labour; first child; attacked on the 3rd day, died on the 8th; child lived.
6. Third child; easy labour; was especially exposed to the infection of puerperal fever; attacked on the 3rd day, died on the 9th; child lived.
7. Lived in town; first child; easy labour; attacked about the 3rd day, died on the 8th or 9th; child lived.
8. Lived in town; first child; easy labour; attacked on the 5th day, died on the 12th; child lived.

I think these are all the cases of puerperal fever, as distinguished from the simpler forms of inflammation. Two of these occurred at the same time when the disease was epidemic through town. Two others when it prevailed in the Rotunda Hospital, but at different times and in different years. None of the labours were difficult or requiring assistance. In seven cases it was the first child, and only one of the children was lost.

9. Was a case of simple hysteritis following delivery by the forceps. It commenced on the 3rd day, and ended fatally on the 10th. It was a first labour.

10. First child, and easy labour; attacked by puerperal mania about

the 3rd day, but though this subsided, the accompanying hysteritis proved fatal. Child lived.

11. Pluripara. Attacked by diffuse inflammation of the back and arms, apparently from a wound of the thumb, received before labour. At least this was Dr. Kirby's opinion, and he published the case.

12. Pluripara. I was called accidentally to this case, because of excessive (accidental) hæmorrhage, apparently the result of vomiting. She was *in extremis* when I first saw her, and by the advice of the late Dr. Charles Johnson I crotcheted the child, and delivered her, but she never rallied.

13. Is the only death I have had from *post-partum* hæmorrhage. The labour was easy and short; then came hæmorrhage; a portion of the placenta was morbidly adherent, and had to be removed. The hæmorrhage apparently ceased, but some hours afterwards I was summoned, and found that internal hæmorrhage had occurred, and she sank.

14. Pluripara. Some narrowing at the brim. Pains very strong, and whilst I was standing by, she suddenly exclaimed that her heart had burst, and in half an hour she died undelivered. A *post-mortem* examination showed a large transverse rent on the left side, involving the cervix.

15. Pluripara. She was attacked by severe convulsions at the beginning of labour. By Dr. C. Johnson's advice I had recourse to craniotomy, as a speedy mode of delivery seemed to afford her the only chance, but the fits continued, and she died.

16. Pluripara. In consequence of delay in the second stage, I delivered her with the forceps, with comparative ease, of a living child, and an hour after I left her, apparently perfectly well, there having been no more discharge than usual. Two hours after I was sent for in a great hurry, and arrived just in time to see her die. I made a careful investigation, but could find neither external nor internal hæmorrhage. There was no *post-mortem* examination. I can only conjecture that it may have been cardiac paralysis or idiopathic asphyxia.

17. This case was called scarlatina; but from what I have seen since, I feel sure it was a case of that purpuric fever, or black death, which was epidemic some time ago. An eruption appeared about the third day, which speedily became maculæ, with large blotches like black bruises here and there. She died about the 6th or 7th day.

In conclusion, I will enumerate some malformations among children which occurred in practice:—

1. One case of acephalous fœtus.
2. Several cases of hare-lip and cleft palate.
3. Two or three cases of spina bifida.
4. One case of large umbilical hernia.

5. One case of six fingers on each hand and six toes on each foot.
6. One case of amputated arm, as described so well by the late Dr. Montgomery.
7. One case in which the first (or nail) joint of each toe (except the great toe) was wanting.
8. Five or six cases of one or both feet being turned up and lying close to the shin-bone.
9. One case of two thumbs on one hand.
10. Two cases of imperforate anus, one of which survived and is alive now.

I fear, gentlemen, I have wearied you with these details, which have nothing of novelty to recommend them, nor even very unusual success. So far as they go I believe they are quite correct, but they might have been more complete; and I would advise all my young friends to acquire the habit, not merely of entering cases into their books as soon after they occur as possible, but to put down even minute points, of which at the time they may hardly perceive the importance. No fact connected with pregnancy, labour, or childbed, however trifling, when added to many others of the same kind, but may acquire a great importance, and aid in solving many of the problems which embarrass us.

The PRESIDENT observed that Dr. Churchill's paper was an important contribution to the literature of obstetric medicine in a department in which it was most deficient in, namely, that of records of private practice. The facts which he had so carefully detailed were of very great importance, and he was sure the paper was one which would take a very prominent place in the literature of obstetrics. It was difficult to take it all in at once, containing as it did so much matter deserving of consideration, and they would only be able to know its value when they came to discuss it hereafter.

DR. M'CLINTOCK had the pleasure of saying that, as Dr. Churchill had told them they might look upon that paper as the winding-up of his professional career in the special branch referred to, there could not be a more appropriate conclusion of a long, active, and successful career in midwifery than the laying before that Society this full, fair, and impartial report of his entire private practice. This clinical record came before them with every qualification that could commend it to their acceptance and approval. They all knew the accuracy of the man that kept it; they all knew the veracity of everything he wrote and said, and they had his assurance that every one of these cases was noted down at the time it occurred, and that the record was in no respect compiled from memory. The cases were noted down not to serve any special purpose

or to make out any particular view; they were accordingly facts of inestimable importance, particularly at the present time, when on some points connected with obstetrics we were greatly in want of accurate statistics of *home practice*. Although Dr. Churchill's active career as an obstetrician was closed, he was sure every one would agree with him in expressing a hope that he might be long spared to them in health to give them, in cases of difficulty, the benefit of his wisdom and of his honest advice. A paper such as Dr. Churchill's embraces so many subjects that he (Dr. M'Clintock) could not note more than one or two just as they happened to strike him. One point was the small proportion of Dr. Churchill's forceps cases. It appeared that the number of cases in which he completed delivery by the forceps was only one in $60\frac{1}{2}$. That must be looked on as an interesting and suggestive fact. No one who knew Dr. Churchill could for a moment suppose that he entertained the slightest prejudice against the forceps. He had always been an advocate for the use of the instrument, but here it was seen that in his private practice it was used only once in 60 times.

There was another important point which he wished to advert to—namely, the manner in which Dr. Churchill managed the placenta. In the third stage of labour Dr. Churchill's practice had been to keep his hand over the uterus, and as soon as it contracted to squeeze the placenta into the vagina, or to bring it within reach of the finger; and the number of cases in which he had to introduce the hand for the removal of the placenta was very small. The practice he (Dr. M'Clintock) pursued was somewhat different—namely, to put the hand on the uterus, but not to make any forcible attempt at squeezing off the placenta until the lapse of 15 or 20 minutes. Without having strictly compared the numbers, he believed he had had on more occasions than Dr. Churchill to remove the placenta by manual abstraction. This would show greatly in favour of Dr. Churchill's method of pressing out the after-birth. He confessed he had held it a point of great importance to keep the hand fairly on the uterus, but to make no attempt to press out the placenta for 15 or 20 minutes.

DR. DENHAM said the Society was deeply indebted to Dr. Churchill for his valuable paper, which needed no praise from him. With respect to the remarks that had fallen from Dr. M'Clintock, he would say that he did not think it was the general custom to wait for 20 minutes. It must be left to every man's common sense and experience to decide when to press off the placenta and when to refrain from doing so. The too early attempt to expel the after-birth was dangerous, to allow it to remain for 20 minutes was perhaps an error in the other extreme.

DR. CHURCHILL was greatly indebted to the President and to Drs.

M^cClintock and Denham for the kind manner in which they had spoken of him. He could not exactly tell them what first led him to the practice adverted to with regard to the placenta. He found that, when he grasped the uterus firmly, it contracted, and expelled the placenta at once, and having experienced the safety of the method he continued to pursue it. He was very watchful for some years after he began the practice—one which was contrary to what he had been taught, yet he could say with perfect accuracy that he had never seen *post-partum* hæmorrhage in any case in which he pursued this method, and he had never had occasion to remove the after-birth by the hand unless there were morbid adhesions. He could honestly say, so far as his memory served him, that it was nearly 20 years since he had had occasion in his private practice to extract the placenta by the hand.

The use of Ergot of Rye.—DR. THOMAS MORE MADDEN said he thought the Society could not but feel deeply indebted to Dr. Denham for having brought under consideration one of the most important practical questions that could engage the attention of the Obstetrical Society, by the exceedingly valuable paper he had read at the last meeting. To Dr. Denham must be given the great credit of having, by his experiments in the Rotunda Hospital, first and finally settled the long-disputed question as to the *modus operandi* of ergot in those cases in which its administration destroys the child. This point being thus authoritatively settled by Dr. Denham, there were two questions remaining for discussion. First—What are the cases in which ergot should or should not be given? Secondly—What preparation should be selected? Now, on these two points he (Dr. Madden) thought that every member of the Society who had had any extensive experience of the administration of ergot, should contribute what he knew to the discussion. From the extravagant terms in which ergot was lauded when first introduced into practice, a revulsion was but a natural sequence, and hence, very shortly after that time, they found the dangerous effects of ergot pointed out in terms hardly less exaggerated than those employed by its advocates. Thus Dr. Hosack proposed to substitute for the name of *pulvis ad partum*, as commonly applied to ergot, the name of *pulvis ad mortem*, as regards the child; and from that time to the present loud and angry had been the controversy on this question. Some practitioners still regard the administration of ergot as perfectly safe during all stages of labour, and this, notwithstanding all the experience of practical accoucheurs to the contrary, including that of the late Dr. Hardy, who showed that in 48 cases, in which ergot was given, the child being alive at the time, it was subsequently and consequently born dead in no less than 34 cases. Notwithstanding this and similar experience, it was not long since gravely advanced as a charge against

the authorities of the Rotunda Hospital, that they did not allow the students there to administer ergot whenever they might think fit to give this drug during labour. For his (Dr. Madden's) part, great as he considered the dangers of instrumental midwifery in unskilful or unpractised hands, he would much prefer to entrust the forceps to a student than to allow him to give ergot whenever he might think fit during labour. In his own practice he used ergot pretty extensively, but he confined the administration of ergot almost exclusively to three classes of cases. First, immediately before putting a patient under chloroform, when about to use the forceps, he almost always gave a dose of ergot to ensure the subsequent contraction of the uterus. Secondly, in cases in which the patient has given birth to a number of children, or in which there is a history of *post-partum* hæmorrhage in her former confinements, he gave ergot when the child's head is emerging from the vulva. Thirdly, he used ergot largely and successfully in the treatment of slight *post-partum* hæmorrhage. He might observe, however, that he had on several occasions seen the placenta delayed by ergot giving rise to irregular or hour-glass constriction of the uterus. Moreover, he now never thought of administering ergot during labour, unless he was prepared to deliver the patient by the forceps within half an hour's time of its administration. The preparation of ergot he most relied on was the fresh powder, given in half-drachm doses, in hot water. The next best preparation was the liquor ergotæ of Dr. Long, which he had often found of great value where the fresh powder was not at hand, and which he now always carried in his obstetric bag. In 46 cases, in which ergot was given, he delivered with the forceps, and in eight of these cases the children were still-born. In 17 cases, in which ergot was given, and the forceps was not used, three children were still-born. One was still-born three quarters of an hour after the administration of the ergot. One two hours and ten minutes, and one half an hour. In a large number of cases of *post-partum* hæmorrhage ergot was given; in some of these the hæmorrhage was checked by it. In the remaining cases other means had to be resorted to. The eight cases in which there was a history of *post-partum* hæmorrhage a dose of ergot was given when the head was on the perinæum, and in five of these there was no *post-partum* hæmorrhage. In the other cases there was some hæmorrhage, and in one, in which two doses of ergot were given after the expulsion of the placenta, as well as one where the head was on the perinæum, the most profuse flooding he ever saw recovered from, occurred notwithstanding.

DR. ATTHILL entirely agreed with Dr. Denham that ergot did not exercise any poisonous action on the fœtus. He had never seen death or any injurious effect produced in the fœtus in cases in which the special action

of ergot on the uterus was not induced. He looked on the drug as being either dangerous or useless. If it induced uterine action its exhibition was attended with a certain amount of danger to the child ; if it did not it was useless. He now never gave ergot, except in cases in which, from the previous history of the patient, he was led to believe that she was liable to hæmorrhage. Under such circumstances he would give ergot, with the view of ensuring a good contraction, but even then, only when the head was on the perinæum. As to the use of ergot in *post-partum* hæmorrhage, he differed from Dr. Madden. He never saw a case of the kind where it was productive of benefit. In many of these cases there was vomiting. He was very much impressed by a suggestion of his friend Dr. M'Clin-tock, that it might in such cases be advantageously given by the rectum. In cases of *post-partum* hæmorrhage, the hæmorrhage could not be controlled by cold he would inject the perchlorate of iron, instead of administering ergot, from which he had never seen any good effect. Another reason why he did not give ergot in such case was that a considerable interval must elapse before it produced any effect with respect to the forceps. Although he had been in practice over 20 years he had not employed them in many more cases than Dr. Madden. He did not know exactly what proportion his forceps cases bore to the whole number of delivery, but it was vastly lower than Dr. Madden's. He never once had used the forceps in any case in which he had had subsequently reason to regret it. He should add that when he used ergot he always added a few drops of the liquor strychniæ to each dose, and he believed this added greatly to the efficacy of the drug.

The PRESIDENT observed that in a great many cases of *post-partum* hæmorrhage, when vomiting set in the pulse went down, the hæmorrhage ceased, and the patient got better ; so that he looked on the vomiting that occurred in those cases as rather beneficial than otherwise. He had never seen any decided uterine action produced by ergot in *post-partum* hæmorrhage, though constantly administering it in these cases. With the exception of one patient in his private practice he had not given ergot once in seven years before the birth of the child. In that case the head was on the perinæum, and the uterus became inactive. He knew there was a lax perinæum, and that all that was wanted was some extra force, and he gave ergot then, and it did well ; but with that exception he had not given ergot before delivery in any case. Dr. Higginbotham, in a paper on *post-partum* hæmorrhage, advised the use of ipecacuanha, instead of alcohol. The action, he presumed, was the same that ergot produced. Probably vomiting was beneficial by its mechanical action on the uterus. As to Dr. Churchill's method of immediate expulsion of the placenta, he (the President) had learned it many years ago from Dr.

Churchill, but had ceased to practice it; for his experience was that he had more hæmorrhage—not very alarming, however—when he pressed off the placenta than when he allowed it to remain a short time in the uterus and to be expelled by uterine action. Dr. Barnes' observation as to ergot is important—"You call forth a force over which you have no control." It may go on so rapidly as to injure the mother, or it may act in such a way as to kill the child, whereas if you use the forceps you have the whole control of the labour in your own hands.

DR. ARTHUR RINGLAND said he had stated at the last meeting that he had on one occasion used hypodermic injection of ergotine with great success. Since then he had had no less than sixteen opportunities of using ergotine, and in every case, with one exception, with the greatest possible success. The quantity he used was from three to six grains, never more than the latter, and generally from three to four. The time in which it acted varied from sixteen seconds to a minute and three quarters. The case in which it failed was not one of labour, but a case of hæmorrhage in a young unmarried woman, 28 years of age. At the suggestion of the President he tried the ergotine, and it gave no signs of action; and he then used sulphuric acid, and it too failed. The first symptom produced by ergotine was vomiting, lasting from fifteen minutes to three hours, but never so much as to give him uneasiness. It was never administered until after the expulsion of the placenta. In the case he (Dr. Arthur Ringland) referred to at the last meeting he injected in the arm. The next case he had after the meeting he injected in the abdomen, and it gave rise to a nasty sore; and on the third occasion, he tried it, at the suggestion of Dr. James Little, in the gluteal region, and it did admirably. If, however, the woman's arm was in a tolerable condition as to fat he would inject it there.

DR. M'CLINTOCK said Dr. Denham had done good service in bringing before them the subject of ergot of rye. It was of fundamental importance to establish that ergot was capable of injuriously affecting the child; and that the way in which it did so was by the uterine contractions which the ergot produces. If this were borne in mind it would serve as a key which would explain, and go far to reconcile all the conflicting statements on this question. Dr. Uvedale West, than whom there was no more painstaking observer, had most carefully recorded 173 cases in which he had given ergot of rye. In some of these cases hours elapsed before the child was born, and yet no harm was done to mother or child. When the cases were carefully analysed the reason would be seen why the foetus escaped injury; either the membranes were unruptured when the drug was given, or the child was delivered so soon after the administration of

the ergot that there was not time for it to do any harm, so that, in fact, there remained only 12 cases that had any value or weight in showing that ergot did not injuriously affect the child. He believed it was incontestably established that ergot was capable of injuring the child, by virtue of the uterine contractions it induced. Dr. Denham, in his former paper, published in 1851, tried to dispel the idea of ergot acting as a poison, and he quoted him (Dr. M'Clintock), Dr. Beatty, and others in his former and present papers as supporters of the view which he opposed. It was due to himself to say that although he did hold that view at one period, yet that so long back as 1865 he read a paper before that Society, in which he undertook to prove that the action of ergot upon the fœtus was purely mechanical, and dependent altogether upon the uterine contractions which it excited. That paper was published in the number of the *Dublin Quarterly Journal* for May, 1865. He (Dr. M'Clintock) did not consider ergot a dangerous remedy when properly used. The golden rule, never to be lost sight of, was not to give ergot unless the child was so circumstanced as that the forceps could be applied. As to its employment for the prevention of hæmorrhage, that was one of the most useful applications of ergot of rye. Not long ago he had a patient, whom he had attended on two previous labours, and on each she was reduced to death's door by hæmorrhage. On the last occasion he gave ergot before the head came on the perinæum, and she escaped without losing a particle of blood. He should say, however, that for several days before her confinement he gave her small doses of gallic acid, three grains three times a-day. The use of ergot as a preventive of hæmorrhage was of great value, and it was but just to an old colleague of his, the late Dr. Hardy, to claim for him the merit of first publishing its utility in that respect. Dr. Hardy's essay on the subject appeared in the *Dublin Journal* for May, 1845, and Dr. Beatty's paper was published exactly twelve months later. This he had alluded to in his memoir of Dr. Hardy. He (Dr. M'C.) constantly used liquor ergotæ, and on two or three occasions he gave it as an enema, and it acted well, and was not followed by any sickness of stomach. As an emmenagogue he had tried it on several occasions, but never found any beneficial effects from it. With regard to the use of ergot in uterine fibroids, Dr. Denham said its action was very unsatisfactory; and such was his own experience, as it had never seemed to restrain the hæmorrhage, which is so constant and so troublesome an attendant upon those growths.

DR. DENHAM, in reply, said he had but little to object to in anything that had been said that night. With respect to the way in which Dr. Madden gave ergot of rye, that was not the practice he (Dr. Denham) had been in the habit of following, except in cases where *post-partum*

hæmorrhage was to be feared from the experience of former labours. If the patient was in a condition to require the forceps he would not deem it an advantage to give ergot before using the instrument if they were correct in the idea that it killed the child by uterine contraction. They must not lose sight of the fact that there were many cases where the woman and her friends had an objection to the use of instruments, and the obstetrician was obliged to sit patiently for hours, and await the efforts of nature, when, if permitted, he could deliver the patient in a few minutes. In these cases he could well understand that ergot would have a useful effect. As to the vomiting induced by ergot, it was not the vomiting that produced the beneficial effect; it was the lowering of the heart's action, and the consequent diminution of the force of the circulation that produced the hæmorrhage, and allowed a coagulum to form.

The Society then adjourned.

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